

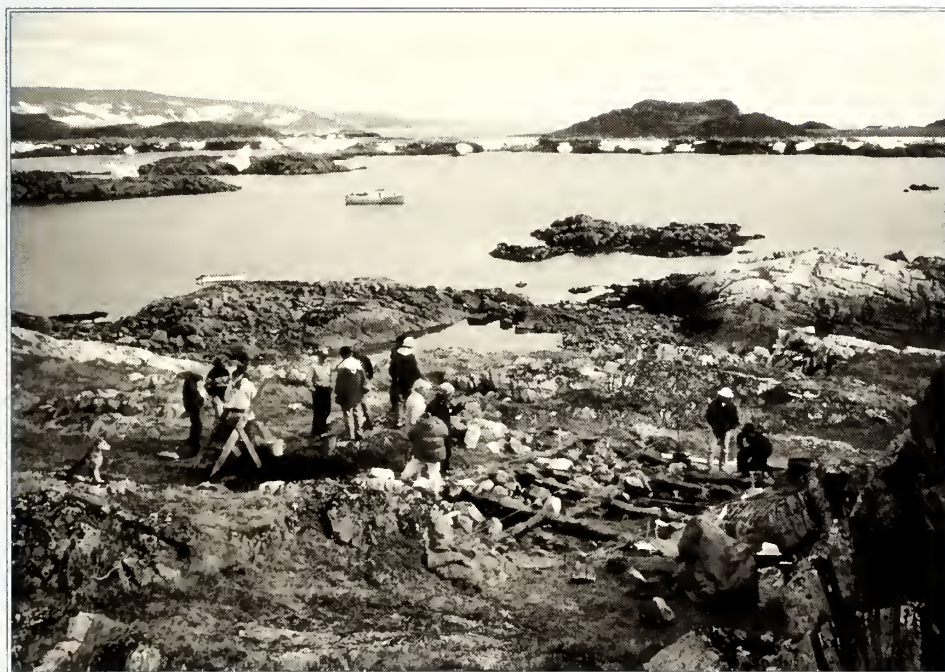
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Honoring our Elders

A History of Eastern Arctic Archaeology



William W. Fitzhugh,
Stephen Loring, and
Daniel Odess, editors



HONORING OUR ELDERS

A History of Eastern Arctic Archaeology



Part of the festivities during the 1993 Elders conference included this presentation by Graham Rowley (L) to Elmer Harp, Jr. of a bar towel inscribed with his name.

Honoring our Elders

A History of Eastern Arctic Archaeology

WILLIAM W. FITZHUGH, STEPHEN LORING AND
DANIEL ODESS, EDITORS



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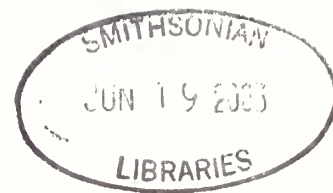
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THIS SERIES IS MADE POSSIBLE IN PART BY THE JAMES W. VANSTONE (1925–2001) ENDOWMENT

*Front Cover: Smithsonian members of the Meta Incognita Project excavating at Kuyait in July 1991, photo by William W.
Fitzhugh*

*Back Cover: [L to R] Father Guy Mary-Rousselière, Frederica de Laguna, and Graham Rowley at the Elders Conference,
Dartmouth College, in 1993, Arctic Studies Center photo archives*



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dedication



THIS VOLUME IS DEDICATED TO
ELMER HARP, JR.,
GENTLEMAN ARCHAEOLOGIST,
WHOSE TRAINING OF STUDENTS AND
CONTRIBUTIONS TO ARCTIC AND SUBARCTIC
ARCHAEOLOGY MADE HIM A LEGEND IN HIS
OWN TIME.

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GRAHAM ROWLEY, a pre-World-War-II explorer of the Central Canadian Arctic, became fascinated with Inuit culture and archaeology and pursued a life-long avocation in this field. His excavations at Abverdjar and other Igloolik area sites were influential in the establishment of the Dorset culture concept.

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figure list and credits

Note: Photographs on the title page of each chapter are pictures of the author(s) taken during the Elders Conference at Dartmouth College by Arctic Studies Center staff unless otherwise noted below.

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Harp's "APA-QUE 1967" team at Wiachouan River Falls: (standing, L-R) Elmer Harp, Jr.; John Miksic; Jack Rinker; (kneeling, L-R) Douglas Harp; William McCarty; William Fitzhugh; William Cavaney

preface

WILLIAM W. FITZHUGH

Elmer Harp, Jr., Dartmouth's Dorset Pioneer: A Personal Reflection

There comes a time when you look back at where you've been and wonder how your life took the shape it did. Sometimes that reverie takes years to come into focus, because we usually owe our paths—or at least the inspiration to move in a certain direction—to unexpected people who intersect our lives. In my case, I followed in my father's footsteps to Dartmouth, and there, after three years of searching for a suitable intellectual home, I discovered Wilson Hall, the Anthropology Department, Mary Westbrook, and Elmer Harp, more or less in that order.

Wilson Hall housed the Dartmouth College Museum, a crusty old brownstone on the corner of the campus green which began to look exceedingly dowdy after the construction of the avant-guard Hopkins Center in the early 1960s. But it had interesting—if also dowdy—exhibits of animals and artifacts. When I arrived on the campus in 1960, the newly created anthropology program was not quite yet a full academic department. Upon stepping through the doors of Wilson Hall I found myself enveloped in a kind of homecoming experience. Part of that was the feeling of timelessness inspired by natural history and cultural displays frozen behind glass. But I suspect more influential was departmental secretary Mary Westbrook's embrace of students—and the tea and cookies she always had available on the sideboard. Mary was Harp's secret weapon in the emancipation of anthropology from the Sociology Department; students walking through her office signed up by the dozens!

For a small undergraduate college, Dartmouth anthropology in 1962 was burgeoning with activity. Although it only had three full-time faculty (Elmer Harp, Jr., Robert A. McKennan, and Alfred Whiting) when I

arrived, Harp and McKennan had succeeded, over a fifteen-year period, in building the foundation for an exciting fledgling anthropology program. Harp was the prime mover behind this effort, which began with his first formal association with Dartmouth in 1946. Over the years, Harp and McKennan (known to us students as "Upper Nabesna Bob" for his inability to speak more than two sentences without mentioning Nabesna, the location of his major ethnographic research in northern Alaska) put together a strong academic program with course offerings in general anthropology, archaeology, ethnology, and physical anthropology, bolstered by field opportunities connected with their ongoing ethnological and archaeological research programs in Alaska and Canada. Other important assets were Dartmouth's inter-departmental Northern Studies program with its staff in geography, geology, and biology; the venerable Dartmouth College (now 'Hood') Museum, which offered students a chance to work with collections and produce exhibits; the Army Cold Regions Research and Engineering Laboratory (CRREL) with its large staff of northern experts who offered occasional courses in physical sciences; and the memorabilia and legacy of Vilhjalmur Stefansson, who had been in residence at Dartmouth from 1953 until his death in 1962 and whose archive and library was a treasure trove of materials as well as a gathering place for northern scholars.

The success of this program, and of Dartmouth's efforts in northern studies in general, is notable. Although Brown University, University of Pennsylvania, University of Wisconsin, and University of Alaska Fairbanks also developed degree programs that sent students on to professional degrees in northern studies, Dartmouth is the only one to have launched careers in northern anthropology without a graduate program. Even more unusual is the large number of sociology and anthropology majors who went into other

areas of anthropology, even before the department began to grow and diversify following the appointment of James Fernandez in 1964 and before achieving departmental status in 1967. Much credit for this successful entrapment of young, frequently distracted minds is due to Elmer's skill at combining teaching, fieldwork, and research programs that appealed to adventurous college students.

Elmer's teaching and organizational activities have been paralleled by a remarkable career in exploration and research. Supported by grants from the National Science Foundation, the American Philosophical Society, the Office of Naval Research, Arctic Institute of North America, and with international training acquired through a Fulbright Fellowship to Denmark and post-doctoral work in Soviet-area studies at Harvard, Harp was familiar with the entire circumpolar region. His special interest was the development of Dorset culture, which he researched in its southern ranges along the forest-tundra boundary from the Yukon to Newfoundland, exploring regional adaptations and technology, demography, settlement patterns, and cultural relationships to Alaska and to neighbors to the south. He began his field studies in the Yukon region of northwestern Canada as assistant to Frederick Johnson of the R. S. Peabody Foundation, and later worked in the Strait of Belle Isle and in western Newfoundland at the southeastern extremity of the Dorset region, along the Thelon River in Keewatin, and on the east coast of Hudson Bay. Harp's investigations compiled the most comprehensive data on Dorset settlement and adaptation in the southern part of its range. Although most of this work is not yet completely published (see below for a bibliography of Elmer's work), it answered one of the outstanding questions of the day, showing conclusively that Dorset culture remained a very conservative entity throughout its two thousand year history and that, despite adapting to subarctic habitats, it remained resistant to and largely isolated from, southern influences. It also provided the most complete documentation on Dorset settlement and dwelling types in these regions at a time when little was known about Dorset household and settlement patterns.

Elmer had a passion for photography and was receptive to new technologies and applications. He recorded his field expeditions carefully with his trusted Leica and did most of his own artifact illustrations. He developed a field documentation system using Polaroid photographs that provided much more detailed infor-

mation than the sketch maps commonly used by archaeologists. His passion for photography extended to contemporary subjects, and his many sensitive photographs of Port au Choix and its people in the 1950s-60s will soon be published in a book edited by Priscilla Renouf. Stimulated by his colleague Jack Rinker, an environmental scientist and air-photo analyst working in Hanover at CRREL, Elmer spent ten years exploring the application of multi-spectral and multi-scalar aerial photography to problems of archaeological reconnaissance along the east coast of Hudson Bay and published several papers on this subject. Although in the end Harp's aerial imagery proved less efficient than ground surveys in northern regions, it proved an innovative method for integrating archaeological data into environmental settings. His work helped set the stage for more intensive applications of remote sensing using aerial and satellite imagery that anthropologists, archaeologists, and ecologists began to use when multi-spectral imagery became available for non-military purposes after 1970.

Elmer was wonderful to work with in the field, and it is here that many of his students best remember him. James L. Farley, writing at the time of Elmer's retirement from Dartmouth in 1978 (Farley 1978:40) captured the image many of us share:

a trim, compact man with a full head of white hair above a surprisingly youthful face....An inveterate pipe-smoker, Harp has the calm, unflappable air that novelists invariably associate with the briar-bearing set. One can easily picture him imperturbably shooting an Arctic rapids with pipe firmly but insouciantly in mouth.

There were times, however, when steel glinted through that unflappable demeanor, as I once discovered, much to my chagrin. Elmer's notes were legion and were widely recognized as models of perfection. How, we wondered, did he manage to keep such detailed and elegant notes, written and drawn in different color pencils and ink, and make such fine sketch maps in the field? So precise and defined were they that he rarely needed the assistance of a professional artist and often prepared for professional meetings by making slides of pages of his field notes. During the summer of 1967, when I was with Elmer on his first season surveying the east coast of Hudson Bay and was desperate to make a good impression, I would see Elmer sitting in the cook tent working on his notes

until late at night. One particular night when we were in Richmond Gulf I stayed up later than he and took responsibility for tidying up the tent before retiring to our smaller sleeping tents. We had been brewing tea on a Coleman stove on the cook tent floor that evening. At about 5am we were roused by a sudden squall that struck hard, leveling the big tent. When Elmer crawled in to retrieve his notebook he found it soaked with tea. The damage was not severe, but I caught a bit of that Harp glint and verbal sting for my carelessness. Needless to say, the notes—and the incident—made a big impression on me.

Elmer's other half—the “better half,” he likes to say—is Elaine. Actually, I hesitate to write about Elaine here, knowing full well that I will never hear the end of it. But a few words are needed as Elaine has been such an integral part of Elmer's life and career that one simply cannot refer to them separately. Beginning with finding Elmer his job at Dartmouth when he was serving as a PT boat officer in the Mediterranean and the Pacific in 1944-45, Elaine has been instrumental in almost every phase of Elmer's professional life. An indefatigable conversationalist and letter-writer, she complements Elmer's penchant for reticence and year after year presided over social gatherings at their home for the entire department and its students. Elmer's role at such gatherings was of course to follow Elaine's instructions, but his principal duty from the students' point of view was to serve his renowned “harp lager” home brew. These gatherings were crucial in establishing the *esprit-de-corps* that became a major part of our anthropology experience at Dartmouth.

Like Elmer, Elaine had definite ideas about field operations and how they should be managed. My brush with her ‘steel’ came early in our relationship when I appeared at their door on the eve of the departure of Elmer's 1963 expedition to Newfoundland. I was active in the Dartmouth's Ledyard Canoe Club, and that year the arrival on the campus of Jay Evans as a college advisor to the club resulted in a number of us taking up kayaking for the first time. Jay had a mold, and I had just finished building my first kayak, a rickety contraption of fiberglass, aluminum tubing, and vinyl. I had not mentioned it to Elmer previously, but there it was, sitting on my car ready to go to Newfoundland, where I planned to spend evenings and weekends paddling

around the coves in Port au Choix, imagining myself an ‘Eskimo’. Elmer did not have much to say that night as we finished packing his trailer, but when I returned early in the morning I learned from him that I could not bring the boat. “Too dangerous. You're all by yourself. Do you know how cold that water is? You'll only last ten minutes even with a life jacket on.” That was it! The kayak stayed home, and I got my first taste of archaeology for three weeks before leaving Newfoundland for an NROTC training cruise. Later I learned that after I had left their house that night Elaine had ‘put her foot down’ most emphatically. Elmer of course had his own qualms. Although this seemed a terrible injustice at the moment, I realized the wisdom of this decision as soon as I reached Port au Choix. Suffice to say that Elaine has been much more than a wife, a mother of four, and Elmer's staunchest supporter. A member (sometimes cook) of several field expeditions and a companion and sounding board for life, Elaine enriched the ‘Harp experience’ that so many students and colleagues came to know and love.

Elmer has indeed become a legend in his own time. To his students he was a patient and gifted teacher who taught by cracking open the door to anthropology and northern studies just wide enough for us to perceive the glimmer and go for our prize under our own steam. Always supportive and perceptive, he taught us to think well, write clearly, and act judiciously. His and Elaine's lives and decades-long partnership has been an inspiration. Artist, photographer, painter, and a poet, too, he will forever be the gentleman scholar from Dartmouth whose teaching, love of the North, and investigations of that mysterious Dorset culture will long be remembered. He in turn remembered the spirits of their world one Christmas eve by lighting a candle on a beluga sternum bone he had found at the Tuurngasiti site in the Belcher Islands and offering the following “Ode to the Ghost from Tuurngasiti”:

Hail, briny spirit!
Once denizen of Arctic deeps
But now inhabitant of some cetacean heav'n,
We honor you with votive light!
Come be with us this Christmas night.
Attest the universal thread of life:
Organic molecules in diverse chains
Link us with you.
And we are kin.

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introduction

WILLIAM W. FITZHUGH
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In addition to Elmer Harp, this volume honors our recently departed elders William E. Taylor, Jr. (1927–1994), Father (Atata) Mary-Rousselière (1913–1994), and Moreau S. Maxwell (1918–1998) who were with us in Hanover, and to Count Eigil Knuth (1903–1996) who could not attend. Their dedication to archaeology, to their students and colleagues, and to northern peoples have helped bring past and future together. Their passing marks a generational change that lies at the heart of our present enterprise.¹

The loss of traditional knowledge with the death of Inuit elders has left a seemingly irrevocable void in our ability to understand the Inuit past. More than any other factor, it was this loss—multiplied regionally among non-Western societies throughout the world—that provided the rationale for the origins and growth of American anthropology in the nineteenth century. In Europe, race and social theory dominated anthropology's formative era. But in America, beginning with Louis Henry Morgan's work in the 1840s, "salvage" ethnology drove the expansion of anthropology as scholars sought to record and collect information and artifacts from native cultures before they vanished forever. This volume confronts a similar time-dependant loss, but reverses the object of study. Here, it is the anthropologist rather than the Native with whom we are primarily concerned. It is not so much the Inuit and northern Indian past as the history of arctic archaeology that is the subject of this book.

Defining the Path

This task would be a worthy goal in and of itself because there is effectively no history of Eastern Arctic archaeology in the formal historiographical sense. For more than one hundred years, travelers, native scholars, and scientists have probed the mysteries of Inuit and northern Indian history seeking clues to their origins and diversity. It has only been since World War II that Eastern Arctic archaeology has come of age with a virtual explosion of professional activity. Taking advantage of the wartime establishment of military bases and government facilities, and their continuation through the Cold War into the current period of industrial expansion and government growth, archaeological exploration has reached the farthest, most inaccessible reaches of Greenland and the Canadian High Arctic. Armed with an increasingly powerful array of scientific techniques and interpretive systems, researchers have recovered finds and information not even dreamed of by the pioneer archaeologists of the early 1900s.

This volume, however, is not about these discoveries but rather is about how these discoveries were made and who made them. It seeks to pay tribute to the intellectual climate and the heritage resulting from this work. It is about the organization of early research efforts in a remote region of the world and about the pioneering scientists who found a way to penetrate the arctic vastness to find testimonials of a remarkable past. This book is an explicit attempt to

stimulate a formal history of the development of arctic archaeology largely because this field has no formal history and, until recently, has shown little interest in developing one. As we experience the transformation of our own scientific tradition and heritage through the passing of our archaeological mentors, we lament the loss of knowledge that is tied to our discipline's oral history. This is a somewhat ironic situation for scholars and academics whose lives have been devoted to producing written words. What can be said when our efforts to document our own history as archaeologists and anthropologists have shown so little progress that our history as individuals (biography) and collectives (disciplinary history) is nearly nonexistent?

Our purpose has been to gather some stones for a foundation—some pieces of archaeological history—that can be used as building blocks for the future. While we realize that the present contributions have gaps and inadequacies, and in many respects barely touch the exciting, preposterous, and flagrant misdirections of our past, at least it may serve as a prod to preserve the story behind the research so that future researchers will have a better understanding of how arctic archaeologists of the late twentieth century constructed the past.

Our region of focus is the Eastern North American Arctic—that portion of North America that lies primarily north of the boreal forest beginning approximately at Herschel Island near the U.S.-Canadian border and stretching east to Greenland and south across the arctic islands and mainland tundra to southern Hudson Bay, Labrador, and Newfoundland. The boundaries of the region are not ironclad but conform to the geographic region that arctic-adapted peoples have traditionally called home, approximating the southern boundary of arctic pack ice and its characteristic inhabitant, *Odobenus rosmarus*, the walrus. In some areas and at some times, this region has included portions of the boreal forest where arctic waters have chilled the coastal regions and have provided seasonal ice cover that have given arctic peoples a means of winter

sustenance. In Labrador and Newfoundland, this ecological zone extends far south of the terrestrial tundra boundary as a result of the influence of the Labrador Current. Here, Inuit peoples have found homelands well south of the Arctic zone proper, no matter which definition one chooses.

This book emerged as an outcome of a conference held at Dartmouth College in Hanover, New Hampshire, on April 22–24, 1993. The immediate purpose of that meeting, and now this volume, was and is to honor a generation of archaeological elders. As it happened, Elmer Harp, Graham Rowley, and Father Mary-Rousselière, who had simultaneously reached the august age of eighty at the time of the “Dartmouth Elder’s Conference,” found their careers straddling what can aptly be called the “golden age” of arctic archaeology. Trained by or associated with the pioneers of arctic anthropology and archaeology like Therkel Matthiassen, Kaj Birket-Smith, Helge Larsen, Henry Collins, and Diamond Jenness, and having contributed by scientific works and training of students to a vast expansion in knowledge, they presided over a field that has changed dramatically in past decades. The archaeology we practice today in the Eastern Arctic has roots in the past; but its conduct, its methods, and its goals have become so different, so fast, that it is impossible not to recognize a “sea change” and the inauguration of a new phase of archaeological enterprise.

Following the pioneering and developmental stages of fieldwork and analysis, we now stand at a threshold in which the goals and methods of archaeology, and field science in general, are changing. Empirical models of science at the foundation of the discipline have reached a point at which further progress can only be made by reexamining fundamental principles and priorities: Who does archaeology? And for what ends? What are its political applications and ramifications? What are the social obligations of research? How can results and benefits be spread equably among various interest groups? Our intention for our own

Elders Conference was that it would serve as a forum to examine the historical development of the discipline as well as chart its future directions. Some of these ideas are explored in the following pages. As background for these papers, we have prepared the second half of this introduction as a discussion of some of the major themes that have motivated the past fifty years of archaeological research in the Eastern North American Arctic.

New Context for the Past

Knowledge provided by archaeologists has resolved some of the questions that have intrigued Western scholars since the Inuit were encountered, first by Norse about 1,000 years ago, and later by Martin Frobisher and other European explorers beginning in the late sixteenth century—questions about cultural origins and history, and the social and economic relations between regions and cultures. In the process, archaeologists to a very real degree found they had inherited the mantle of the early explorers, finding their “northwest passages” and “farthest norths” on the gravels of Independence Fjord and in the cache boxes of abandoned Thule winter houses. By now, hundreds of sites have been recorded from which thousands of artifacts have been collected, cataloged, and removed to distant museums. Among these finds are the humblest of implements—broken needles, flakes of chert, amber beads, and other items that testify to a human presence in the Arctic. Other finds, like the Thule carving of a Norse man from southern Baffin Island, Norse chain mail and wool textiles from a tent floor on Bache Peninsula, a Dorset soapstone polar bear from northern Labrador, and the Pre-Dorset ivory maskette from Ivujivik stir the imagination and have earned international respect for the dynamism and ingenuity of Eastern Arctic cultures and their heritage. While cultural developments in the Eastern Arctic may not rival those of the more resource-rich Western Arctic, archaeology has demonstrated the independence and unique creativity of these arctic peoples.

These finds represent the remains of 4,000 years of arctic history from a host of different cultures spread over an immense geographic region. The archaeologists who gathered these materials have produced an equally impressive array of scientific publications. Whereas the library available to our elders in 1945 occupied perhaps a couple of shelves dominated by reports from Greenland, today’s Eastern Arctic archaeologist has a room full of books and journals and file cabinets filled with published and unpublished reports. Albert Dekin, who amassed the most comprehensive bibliography of arctic archaeology as of 1976 (Dekin 1978), listed more than 1,600 publications, and since then the literature has more than doubled. During this period, the number of archaeologists working in northern Canada and Greenland increased from a small cadre of what Moreau Maxwell has called “chapped hands” pioneers to dozens of professionals, students, and enthusiasts hailing from the United States, Canada, Europe, Japan, and elsewhere. To all appearances, the results of the past fifty years of archaeological research in the Eastern Arctic have been successful beyond measure.

Old Faces—New Questions

From time to time, other voices were heard expressing interest—and concern—about the growing archaeological industry in the North: these were the voices of the Inuit, of community leaders, of political activists, and occasionally of scholars. Increasingly, Inuit were asking questions that were not easy for scientists to comprehend—questions about rights of ownership over artifacts and heritage; about archaeological authority versus traditional belief and oral history; about the disturbance of ancient habitations and of the resting places of shamans and ancestors; and about the bones of ancestors removed to museums and universities for study or exhibition. For the most part, archaeologists listened respectfully but in the end continued their traditional research pursuits. For Western scientists, knowledge was valuable for its own sake

and would provide the Inuit and others with a tangible “real” history.

By the 1980s, it was clear that a conflict in values was in process. Archaeologists had largely approached their work from a Western cultural and scientific perspective that made little sense to the Inuit. Where archaeologists saw sod houses and tent rings, Inuit read these same structures as dwelling places of their ancestors, and in some cases knew them as the homes or burial places of their parents and grandparents. When archaeologists removed artifacts from ancient houses and villages, Inuit saw this theft of their heritage as serving only the interests of southern museums and the careers of outsiders whose books they rarely saw and could hardly understand. Ancient sites and remains had existed forever in the minds of the Inuit. They had played among them as children and had found relics in the walls and floors of their old houses and tent sites. They had collected artifacts as curiosities and had learned about their ancestors by listening to elders speak about memories stimulated by chance finds of old harpoons and artifacts. For the Inuit, the remains of the past lived among them and inspired them, and they felt they should remain with them.

This clash of values did not erupt overnight; it accumulated over the years as Inuit people watched archaeologists come and go with their instruments, photographs, and boxes of specimens. In the early days of research in the North, circumstances required archaeologists and local people to live and work so closely together that the contrast between Western and Inuit values was less apparent. But in recent decades, with the arrival of aircraft and radios, archaeologists and other scientists, government administrators, inspectors, and specialists of all kinds flooded the North, and the local personal bonds that once existed between Inuit and visitors has weakened. Gradually, Inuit began to see that if archaeological work was to continue, it had to continue on terms more favorable to Inuit interests. The pursuit of archaeology had to become something valued by the Inuit as well as by outsiders.

During the past twenty years, a growing movement to realign the goals of archaeology with the aspirations of native people has taken root. Native resistance to external administration, and to southern appropriation of northern cultural resources, has led to a variety of new governmental structures, including the establishment of Home Rule in Greenland and Nunavut in Canada. Recent land claims agreements with northern native groups include provisions for regulating access to cultural resources. During the past decade native groups—the Labrador Inuit Association, Inuit Tapirisat of Canada, the Alaskan Federation of Natives, and regulatory groups like the Inuit Heritage Trust—have instituted policies that influence or regulate government permitting structures in favor of local communities. Policy statements establishing standards for ethical conduct of science in the North have been implemented, and community consultation structures that require archaeologists and other scientists to confer regularly with local communities have been created.

As a result of these procedures, local participation in archaeology and environmental science is increasing. Science institutes and support centers have been established in a number of northern towns and cities, and institutions of higher learning, like Arctic College in Iqaluit, have been created to bring education directly to northern residents, with the result that the number of local people actively participating in scientific projects has expanded rapidly. With land claims and new political authorities giving communities property rights over much of the land surrounding their villages, the stewardship and responsibility for “managing” archaeological and historical sites has been transferred to local hands. Soon, many northern villages will have their own culture centers and the larger municipalities will have facilities to house and interpret cultural resources.

These conditions create a new context for archaeology in the North that was not envisioned by our archaeological elders when they began their work during and after World War II. They, like the Inuit elders

they worked with, had to confront many changes in the way they conducted their research. While this span of time has been extremely productive in scientific advances, it has also been a time of great social transformation. The discipline of archaeology, once a kind of arcane knowledge that was largely inscrutable to northern residents, has emerged into a public arena where it has found itself politically vulnerable and increasingly under the control of financial and political forces beyond the “academy.” Whether we like it or not, the old scientific values that motivated the expansion of archaeological research—scholarship, knowledge, excitement of exploration and discovery—are rapidly being replaced by a new politics and a new science in which social relevance and local interest and authority are increasingly central issues of archaeological concern.

Also increasingly important are the purposes to which archaeological knowledge is put. One of the major criticisms made by Inuit of archaeologists is that the results obtained, as well as the finds themselves, have not been made accessible to northern people. To some degree, Inuit still know their past through the medium of oral history as told by their elders. While Inuit are not averse to augmenting this source of knowledge with archaeological and environmental data (Weelaluktuk 1980), they wish to see archaeology conducted in a way that benefits their communities more directly.

For these reasons, archaeologists who have previously communicated the results of their work to scientific peers and government permitting authorities have begun to recognize the need to make their work available to a local audience. As a result, popular books, films, videos, archaeological and historical exhibitions, media programs presenting research results in local schools, and training opportunities for local youth have become important aspects of archaeological research.

In reality, what has evolved over the past several decades is a kind of archaeology that was not envisioned by our elders but is one that has come to

dominate contemporary work and, undoubtedly, will do so in the future as community involvement in archaeology increases. In the process, the role of the archaeologist in generating research questions and capabilities that satisfy scientific peer committees will not be the only criterion upon which the success of research programs will be judged. In this context, it would appear that arctic archaeology as it has been practiced in the twentieth century has already passed into history. While it may be difficult to pinpoint a moment in time when major changes occur in an evolving discipline, it is clear that during the last two decades of the twentieth century the social consequences of arctic science came more into focus. We believe that the quest for a pure intellectual rationale for northern studies has ended and a new paradigm based on community relevance and local participation in all phases of investigation, stewardship, cultural resource management, and educational values has begun.

The discipline of arctic archaeology has come to a divide in the road where the practitioners have met the descendants of the ancestors. Whether this is a crossroads or a convergence remains to be seen, but it does seem an opportune time to look back at the discipline’s own trail to consider a history of the practice of archaeology in the Eastern Arctic in order to retrieve a sense of the practitioners themselves, of the opportunities and issues that motivated them, and their experiences in conducting fieldwork in what is no longer a remote, inaccessible land.

A Rationale for History

Prior to World War II, the Circumpolar North was distant and nearly inaccessible; it was an exotic landscape filled with icebergs, polar bears, and people whom Westerners had come to call “Eskimos.” This was the “heroic period” of arctic archaeology when explorer-scientists arrived by dogsled and boat. In those days, logistical constraints often necessitated spending a year or more with a native host community. Eth-

nology was an important part of an archaeologist's skills then, and one had to learn the language of the hosts and participate in the full round of subsistence activities. Field crews consisted nearly entirely of indigenous hunters and seamstresses, and every aspect of archaeology from discovery to interpretation emerged from this association.

In the postwar years, there was a huge expansion of archaeological and scientific work in the Eastern Arctic at a time when traditional Inuit societies were undergoing rapid change. Relocation, economic stratification, expansion of medical and educational services, introduction of new technology and housing, welfare, and the establishment of a military infrastructure were imposed and had far-reaching impacts. Archaeologists witnessed and experienced many of these changes firsthand, but they have had remarkably little to say about them. Few have published on their observations, and few have seen their notes and photographic records as valuable resources documenting a major period of transition.

After the war, the increased militarization and administration of the Arctic, the ease of logistics brought about by aviation, and eventual village centralization of a previously dispersed population, made access to the outermost reaches of the North ever easier. Most archaeologists were no longer willing to spend a year or more in the remote settings their intellectual curiosity took them to. Rather than hiring local assistants, they began bringing in students and gradually became independent from native communities. And while ever ready to champion the people in whose land they worked, distances emerged as research interests and results diverged from the needs and concerns of native communities.

During the last twenty years, much of the circumpolar north has been transformed. Satellites now bring American and Canadian television networks to every community in Arctic Alaska and Canada. No one lives entirely in the country. And although village elders still retain an identity predicated on subsistence activities

and associated religious practices, for many, especially those born after 1960, their cultural legacy has been obscured.

With the resolution of land-claim negotiations, northern native peoples from Siberia, Greenland, Canada, and Alaska are becoming increasingly empowered to mandate activities taking place on lands under their jurisdiction. Together with their newfound political autonomy, they are demanding a role in various aspects of the production of knowledge concerning their ancestors—especially as concerns archaeology.

Shifting Intellectual Climate

Twenty years ago, Albert Dekin, one of the few scholars to take an explicitly historical interest in the development of arctic archaeology, found himself at a different crossroads:

The winds of intellectual change are blowing across the Arctic. This is a time of interpretive and synthetic flux. It is clearly not a time to draw historically meaningful lines between periods of intellectual development. Perhaps it is best to seek evidence of interpretive change and to point in directions toward which Arctic archaeologists may be tending. (Dekin 1978:159)

In outlining future prospects for arctic archaeology, Dekin pointed toward the broadening of perspectives to include modeling, human-environmental relationships, climatic influence, sampling strategies, growing governmental involvement in cultural resource management, the increasing residence of archaeologists in the North, multidisciplinary approaches, and the growth of publication outlets.

While Dekin's primary interest was in demonstrating the scholarly development of archaeology as an intellectual pursuit, two of his comments presaged changes that, by the time of the 1993 Elders Conference, had become watershed issues affecting all aspects of the field: the growth of Inuit and government involvement in archaeological programs, and the increasing use of archaeology as an applied science for training and education in northern communities. In

1994, the glimmerings of a new role for archaeology anticipated by Dekin twenty years earlier had taken center stage, eclipsing a century of dominance of northern research by the scientific paradigm and changing the way many archaeologists approached scientific problems and fieldwork opportunities.² Eastern Arctic archaeology is now collaborative and embraces a variety of voices. Research results also have to meet the scrutiny of community officials and administrators and resource managers closely tuned to local desires and aspirations. Archaeologists have seen their roles change from authoritative purveyors of scientific knowledge to listeners and facilitators. To a large degree, they have become technical advisors working in collaboration with community interests.

Seen in this light, this publication marks a multitude of transitions. As our archaeological elders pass on their versions of our collective history, we all have become aware that far more has changed than merely field methods and research questions. In this case, generational change has coincided with political and social transformations that will forever alter the methods of twentieth century science as practiced in the North. New governmental and financial regimes, permitting processes, institutional relationships, oversight committees, and many other changes are having profound effects on the science of archaeology. Our elders conducted their careers during a time when science was remarkably isolated from nonacademic concerns. Then, the major problems facing the fieldworker often were funding, logistics, and the North's ever-powerful capacity to confound one's best-laid plans. Today our world is smaller. We can still be weathered in; still face unexpected interruptions and delays; and still rely on intuition even as we apply new technology to our work. But as we do so, we are increasingly mindful that arctic archaeology, as it has been defined for more than one hundred years, is now being driven by a different set of interests than those that drove the investigations both of the first-generation pioneers and of the second-generation researchers, our mentors.

It is our intention that this volume enables us to step back into the discipline's past in order to better understand its roots and development. Arctic archaeologists have been remarkably chary to write of themselves and their work from a personal or anecdotal point of view. As a result, the published literature on the history and biography of Eastern Arctic archaeology is sadly undernourished. Few have written about their background or of their field experiences, despite the fact that most maintained field diaries and notes that could enrich scientific knowledge through an informal ethnography of the North, personal experiences of fieldwork, and events that transpired that had a bearing on how they, and we, have come to understand one part of the arctic past. Some notable exceptions include J. Louis Giddings's (1967) *Ancient Men of the Arctic*; Frederica de Laguna's (1977) *Voyage to Greenland*; Stuart Jenness's (1991) *Arctic Odyssey: The Diary of Diamond Jenness, 1913-1918*; Peter Schledermann's (1996) *Voices in Stone*; and Graham Rowley's (1996) *Cold Comfort: My Love Affair with the Arctic*. It would be a tragedy if our accumulated backlog of unreported scientific work engaged our "golden years" to the point of exclusion of personal history and observations.

The Need for a Native Archaeology

The goal of the Elders Conference and of this work also was to recognize past developments and accomplishments as well as new directions. Archaeology has moved into the forefront of issues important to northern peoples today for a variety of reasons. Understanding the past is only one of these. Others, far more influential among the general population of today's changing North, are how the past is studied, interpreted, and presented.

The interest now being expressed by northern people in their past is not only academic; it also has to do with a range of issues stemming from a century of experience with archaeologists and other scientists living in and working out of their communities. And, as

noted above, all too often scientists have failed to inform communities about their research either before or after the completion of their work, or have not taken the initiative to develop relationships with local authorities and community representatives, or have failed to present the results of their findings to local institutions, schools, and media.

The importance of producing popular publications for northern communities cannot be understated, and their absence, particularly in the Eastern Arctic, has encouraged the view that archaeologists and other scientists have pursued careers independent of the interests of local residents. Despite efforts to inform communities, few projects have found the means to communicate the results of their work. Scientific publications are generally too technical for popular consumption; museum collections are far removed from their place of origin and only rarely find their way back to the communities because of lack of interest, funds, or facilities. Even archaeologists who wish to make themselves available for community discussions often make the mistake of scheduling visits during the summer season when local residents are away from their villages at summer camps and fishing stations. Popular publications are rarely designed to engage rural audiences, and few attempts have been made by northern school systems to abstract scientific and historical information for local curricula. The lack of museums in the North and absence of archaeological exhibitions in the south that could produce illustrated catalogs that northern residents could appreciate, and the absence of television or radio programming featuring archaeological discoveries and interpretations have all contributed to the intellectual and social isolation of academic archaeology from the northern public.

Many researchers have spent considerable efforts in maintaining ties with local communities and have sent copies of reports and publications back to villages for local distribution. Most made efforts to have their projects reviewed locally even before this was a

legal or administrative requirement. In the case of our elders, most developed close relationships with local families during the many years of their association, exchanging gifts, bringing northern youth south for schooling, and sending care packages and photographs. But as the years passed and younger generations of Inuit leaders assumed control, these old alliances, and those that developed among the younger generations of researchers, often failed to meet the tests of an increasingly politicized dialogue that pitted northern residents, local authorities, southern scientists, and government officials against one another. In this climate, archaeology has come to symbolize a history of domination and manipulation by the larger society in general. In a number of instances in the 1980s, local communities shut down archaeological programs that had been underway for years, and in doing so sent signals to archaeologists and government authorities that communities would no longer honor unilateral decisions made in remote southern committees on issues of special or even of symbolic importance to them (Helmer and LeMoine, this volume).

Perhaps the most innovative aspect of northern archaeology as it is practiced today is the degree to which research has come to be integrated with community perspectives and concerns (discussed in greater detail below). It's clear that the future of the past will include a variety of voices, archaeologists and historians, hunters and storytellers, Inuit and *qallunaat*, and that our perceptions, insight and knowledge will be richer for it (Nicholas and Andrews 1997).

The Journey: Themes and Approaches

Despite its venerable age, little attention has been given to the documentation and analysis of the history and theoretical development of Eastern Arctic archaeology. In the rush to create an arctic prehistory, the discipline has eschewed a reflective consideration of the history of the field and, in particular, has considered only those historical aspects that have had a bearing on theories of culture history. The earliest papers treating the

history of the discipline are only little more than two decades old (Dekin 1978; Taylor 1977) and treat only classic research themes. Others began to appear only in the 1990s, in the form of festschrifts for prematurely deceased or retiring archaeologists (Gilberg and Gulløv 1997; Morrison and Pilon 1994).

This volume, while dedicated to the exploration of the history of Eastern Arctic archaeology, has not been prepared consciously as formal history. To do so would have entailed a different format as well as a different stimulus for the project. The need for such a study is readily acknowledged; indeed, the archaeology of the North has suffered in its absence. While the fault is ours, few northern specialists have been willing to divert their attention from the expansion of knowledge to the documentation of how that knowledge has been accumulated. Those that have (Dekin 1978; Fitzhugh 1972b; Harp 1964b; Larsen 1961; Noble 1972; Park 1998; Taylor 1968) have often considered the history of regional studies rather than of the North American Arctic or the Eastern Arctic generally (see, however, Collins 1984; Harp 1984). Only Dekin (1978) can be credited with a broader effort. Dekin's work was produced from a bibliographic base, and his 161-page essay alternates stylistically between an annotated chronology of publications and periodic analysis and generalization. His periodization (table 1.1) reflects

general chronological trends and developments in the field, but fails to explore in depth many of the issues and questions that have concerned investigators. Now, with the broad framework of regional culture-history established throughout the Eastern Arctic, is an ideal time for a reassessment and update of Dekin's pioneering work. The contents of this volume are offered more as grist for that mill than to serve as a substantive contribution to a much-needed comprehensive historical analysis. For that reason, we provide only a skeletal history, referenced to only a limited degree and decidedly biased in terms of the authors' lack of experience or familiarity with many aspects of the field.

World War II changed everything relating to arctic archaeology in this region. Dekin's bibliography provides one measure of the change: twelve pages are devoted to pre-1945 activities while 140 pages discuss work from that period to 1976. We cannot here aspire to summarize the fifty postwar years of Eastern Arctic archaeology. Rather, we outline a framework to which the papers presented at the Elders Conference can be attached. The themes have been broken out as follows: (1) origins research, (2) pioneering research, (3) the post-World War II era, (4) expansion and professionalism, (5) the Santa Fe Conference and the "core area" concept, (6) surveys and environmental archaeology, (7) historical archaeology and European-Native

Table 1.1/ Dekin's outline of arctic research periods and persons.

Period Designation	Date Range	Notable Persons
Explorers and Ethnographers	1750-1910	Steensby, Mathiassen, Pinart, Dall
Expeditions and Pioneers	1910-1935	Rasmussen, Mathiassen, Jenness, Collins, de Laguna, Holtved
Chronologists and Prehistorians	1935-1960	Quimby, Knuth, Collins, Harp, Mary-Rousselière, Maxwell, Taylor, Larsen, Laughlin, Freed, Giddings, MacNeish, Meldgaard
Archaeologists and Anthropologists	1960-1970	McGhee, Irving, Dekin, Nash, Tuck, Fitzhugh, VanStone, Plumet, Merbs, Wenzel, McCartney, Kent, Campbell, Hadleigh-West, the Clarks
Recent Period	1970-1978	Arundale, Schledermann, Hartweg, Wright, Gordon, Morlan, Cinq-Mars, Noble, Cook, Workman, Turner, Aigner, Anderson

contact studies, (8) government programs, funding, and resource management, (9) international research and collaboration, (10) bones, stones, and symbolism, (11) synthesis and popular prehistory, and (12) community archaeology. Embedded in this historical discussion are a variety of research themes that include questions concerning ethnic origins and interaction, circumpolar contacts, cultural ecology and environmental modeling, nationalism, the management of cultural resources, and native education and training.

Origins Research

Early European explorers entering the Eastern Arctic for the first time were astonished to discover Inuit people living comfortably throughout much of the region. With the exploration of Alaska and Siberia, it was realized that the Inuit “oecumene” was the largest culture and linguistic area of the world. The peopling of the Arctic remains an extraordinary account of human ingenuity and flexibility, and one of the most vivid testimonies to the hunting heritage of humankind.

To Europeans who became familiar with the new discoveries in arctic North America, the question of the origin of the Inuit became a puzzle of global dimensions. Martin Frobisher, noticing the Asian appearance of the Baffin Inuit and their copper ornaments in 1576, believed he was at the entrance of the fabled Northwest Passage to China. Later explorers looked eastward to the Sami and Samoyedic peoples of northern Russia for Eskimo origins, ideas expanded upon by David Crantz (1767), an early Danish historian and Greenland geographer. However, it was H. J. Rink’s 1887 to 1891 investigations of Greenlandic language and culture that first pointed toward Inuit origins in Alaska (Collins 1984:8).

With no convincing explanation of Eastern Inuit origins forthcoming from Inuit oral history either in Greenland or in Baffin, studies of this question took divergent directions. One group followed the German *Kulturkreiss* school that emphasized diffusion and migration. Similarities between Eskimo technology and



1.1/ Charles Francis Hall and Koo-ou-le-arng on Kodlunarn Island in Frobisher Bay, Nunavut.

finds from the Paleolithic and Mesolithic cultures of Western Europe prompted some researchers to link Eskimo origins with the northward-advancing peoples who followed reindeer and Ice Age mammals into the arctic zones of northern Europe and Russia (Dawkins 1874). In the meantime, Danish scholars began to recognize two phases in the history of Greenland: an early stone age culture found prevalent in the deep middens of West Greenland (Solberg 1907) and a more recent culture resembling that of modern Greenlandic Eskimo. Subsequently, Danes advanced a series of theories of “Paleoeskimo” and “Neoeskimo” origins in North America based on linguistic, geographical, and ethnographic reconstructions (Birket-Smith 1929; Hatt 1916; Steensby 1917; Thalbitzer 1904). Similarly, Franz Boas (1902) argued for Canadian Arctic origins with subsequent expansion of Eskimo peoples into Alaska.

An alternate route toward understanding the past—that of archaeology—had been gathering steam in Europe since the mid-nineteenth century. Archaeology provided a scientific method independent of history and ethnology. Arguably, its first application to a specific problem in the Eastern Arctic was Charles Francis Hall’s investigations of Kodlunarn Island in southeast

Baffin Island (fig. 1.1). His work in 1861–1862 linked the remains of roof tiles, ceramic crucibles, coal, European wood, and iron with Inuit oral historical accounts of early *qallunaat* and historic records of the Frobisher voyages (Fitzhugh and Olin 1993; Hall 1865). Further, he sought to preserve his finds and records through full publication and museum storage.

The lost relics of European explorers figure significantly in resolving another historical problem, namely, the fate of the Sir John Franklin expedition. In 1854, while searching for remains of the lost Franklin expedition on Boothia Peninsula, John Rae acquired an array of artifacts that Inuit had collected from the trail of camps left by the doomed Franklin party as it struggled over ice and arctic islands toward the mouth of the Back River (May 1855; Rae 1855). The relics were the first hard evidence of the missing Franklin party to reach England, ending nine years of speculation over their

fate. Subsequent search expeditions found additional traces of the lost explorers. These finds, including relics and skeletons, and eventually a single document preserved in a stone cairn, spelled out the expedition's tragic demise. The recovery of Franklin relics from Inuit who had found them, and others collected during the intensive surface surveys conducted by the search expeditions, are among the first archaeological finds recovered in the Central Arctic (fig. 1.2). During this period, the waters of the Eastern Arctic were crowded with search expeditions that scoured the previously

unvisited central arctic region for traces of the lost party (Sutherland 1985).

Franklin-era exploration also produced the first detailed maps of the Eastern and Central Canadian Arctic, and the expedition narratives were eagerly consumed by the lay and scientific communities of the day for they provided detailed accounts of the physical, biological, and cultural dimensions of the polar world. The narratives also provided numerous references to abandoned Inuit villages and provided the most detailed documentation of the day on Inuit peoples of the Central Arctic. These early accounts, however, always begged the question of Inuit history and origins.

While Hall's report on his activities at Kodlunarn Island qualify him as the first practitioner of "problem-oriented" archaeology in the Arctic, he was soon joined by others with an interest in reconstructing the past. T. G. B. Lloyd's (1874) antiquarian study of artifacts and dwelling re-



1.2/ *Relics of the Franklin Expedition recovered by the British Franklin Search Expedition, 1857–1859.*

main in southern Labrador produced the first published report on this region's prehistory. His work was preceded by mid-century collections of archaeological material recovered from Thule graves and Dorset village sites sent by Moravian missionaries to their brethren in Saxony, England, and Switzerland, but these materials never became the object of scientific study. Tantalizing suggestions of the antiquity of Inuit land tenure gradually accumulated in the wake of the increased pace of scientific interest in the North brought about in part by the collections acquired tangentially

by researchers associated with the First International Polar Year in 1882–1883 (Barr 1985; Boas 1888; Greely 1888). In 1900, Captain George Comer collected Eskimo artifacts from Southampton Island that were analyzed by Boas (1907), and between 1915 and 1917 Comer excavated sites around Smith Sound, including “Comer’s Midden” (Wissler 1918). Solberg’s (1907) analysis of the West Greenland stone age materials was the first study to apply newly developed European standards to “stone age” cultures in Greenland.

Pioneering Research

Two expeditions figure significantly in the early history of Eastern Arctic archaeology. The Stefansson-Anderson Arctic Expedition of 1913–1918 enabled a young anthropologist, Diamond Jenness, to conduct fieldwork in the Western Canadian Arctic and Alaska. Exploring the region between the Coppermine River and Point Hope, Alaska, Jenness and Stefansson excavated archaeological sites that provided the first evidence of deep time depth for Inuit cultures in this region (Jenness 1923a; Stefansson 1914a, 1914b; Wissler 1916).

With the commencement of Knud Rasmussen’s Fifth Thule Expedition of 1921–1924 archaeology in the Eastern Arctic became a fully professional, mission-directed enterprise. Rasmussen’s colleague, Therkel Mathiassen, conducted excavations at several prehistoric Eskimo sites in northern Hudson Bay and found that their basic features linked them to remains previously discovered in the Thule District of West Greenland. Ultimately, however, he surmised (correctly) that the Thule culture had originated in Alaska and believed (mistakenly) that they were the first people to colonize the Eastern Arctic (Mathiassen 1927b).

Mathiassen was proven correct in his assessment of Thule’s Alaskan origins by the work of Diamond Jenness and Henry Collins in Bering Strait, but he resisted for many years the idea that a pre-Thule Dorset culture had existed in the Eastern Arctic, despite the abundance of stone tool midden sites in West Greenland and Canada. This concept had been advanced by

Jenness as a hypothesis based on finds culled from a collection of mixed materials presented to the National Museum in Ottawa (Jenness 1925). Dorset remains were subsequently recognized in many sites throughout the Canadian Arctic and Subarctic. By the beginning of World War II, the existence of the Dorset-Thule sequence had been accepted by all arctic workers, and virtually all believed that Thule replaced Dorset and was the ancestral culture of most, if not all, living Inuit peoples of the region, including Greenland.

The Fifth Thule Expedition inspired other research throughout the Eastern Arctic. In Labrador, W. D. Strong (1930) described the “Eskimo-like” Indian culture of the central Labrador coast; Junius Bird’s 1927 work on the development of Labrador Inuit culture revealed 300 years of contact history with Europeans (Bird 1945); and Leechman’s 1935 excavations in Killinek revealed Thule and Dorset presence (Leechman 1943). In Newfoundland, Jenness (1929b) pointed out Dorset parallels between Beothuk and Dorset harpoons, and Wintemberg (1939, 1940) conducted surveys documenting the presence of both Dorset and Indian sites.

In the Hudson Bay, research by Quimby (1940) advanced the case for a local Manitunik culture that blended Dorset and Indian features but later proved to be a result of stratigraphic mixing of two distinct occupations. And farther north, Graham Rowley (1940) in 1939 excavated the first pure collection of Dorset materials from Abverdjar where he recovered many remarkable art objects (fig. 1.3).

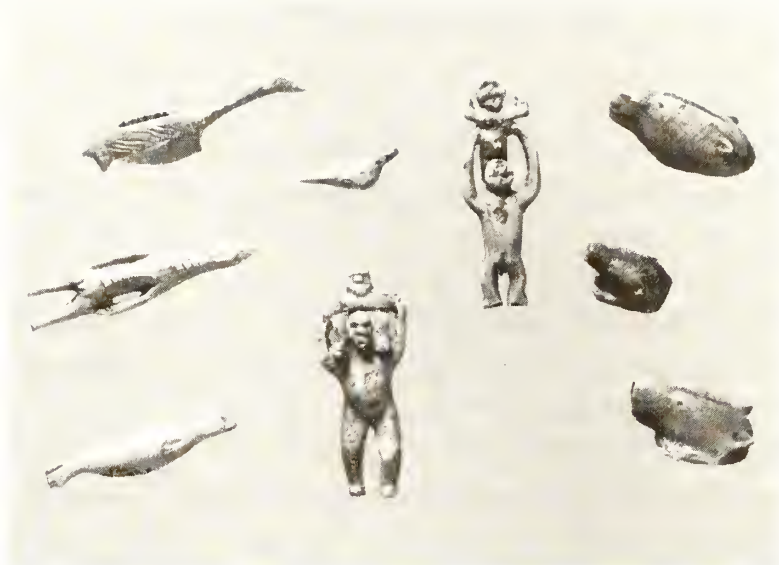
The greatest archaeological activity in the Eastern Arctic in the pre-war period, however, was an extensive survey of Greenland conducted between 1929 and 1935 by Mathiassen, Holtved, Degerbøl, Larsen, and others that resulted in a huge publication output. A corresponding research program by Aage Roussell (1936, 1941) and his colleagues Christen L. Vebæk and Poul Nørland focused on the Viking’s Eastern Settlement and Western Settlement.

One aspect of the growth of Eastern Arctic archaeology is in the degree to which the emerging

discussion of Dorset culture figured in schemes of Northeastern prehistory. As regional sequences were defined throughout the eastern United States and adjacent Canadian provinces, investigators were challenged by increasing evidence for a far greater antiquity for their prehistoric assemblages than had previously been assumed and for increased evidence of interregional contact, influence, and exchange. A symposium held at the American Anthropological Association meetings in Andover, Massachusetts, in 1941 culminated in *Man in Northeastern North America* (Johnson 1946). This was perhaps the first

symposium that sought to place Eastern Arctic cultural developments in the wider context of Northeastern archaeological and ethnographic traditions; it was also the last meeting in which cultural anthropologists and ethnologists worked closely together at archaeological interpretation. While this was an important step forward from a past that had been overly dependent on Boasian and Danish ethnographic schools, it thrust upon archaeology a scientific, technical, and quantitative paradigm that failed to appreciate the role of culture and behavior. The importance of the meeting was to present arctic archaeology as a fully professionalized field with a deep commitment to continental-scale integration.

This brief review finds Eastern Arctic archaeology at the threshold of World War II with the bare outlines of its culture history identified. Only in Greenland were there substantial quantities of excavated collections and published reports. Dorset culture was known throughout the region, including in subarctic Labrador and Newfoundland. Pre-Dorset was not yet a glimmer in the archaeological eye, and Thule was known in several regional and developmental stages—from the Norse-influenced Inugssuk culture of West Greenland to the historical era. Environmental studies had not yet



1.3/ Postcard of Dorset carvings from Abverdjar, collected by Graham Rowley in 1939

been applied to archaeology, and relationships to Alaska and beyond remained unknown and unexplored.

The Post-World War II Era

After 1945, archaeology in the Eastern Arctic advanced at a rapid pace. Using the newly established military bases and government infrastructure, and—significantly—air transportation, archaeological research in the North was transformed. Another important institutional change had occurred at this time—the creation of a national archaeological program organized by Diamond Jenness at the National Museum of Canada. Jenness invited Henry Collins, a friend and colleague since Jenness's early work in Bering Strait in 1926, to begin archaeological work in the Central Canadian Arctic. Between 1948 and 1955, Collins led expeditions to northern Hudson Bay, Southampton Island, and Baffin Island. He confirmed Dorset antiquity and established its first developmental sequences. At Crystal-2 and Resolute, he found ceramics and Penuk-like harpoon heads that proved Early Thule links with Alaska, and iron tools that came from Greenland's Cape York meteor fall. And at the near end of the timescale, he speculated that the ethnographic Sadlermiut might be a relic Thule-influenced Dorset population. Among Collins'

most important contributions was providing the first arctic field experience for William E. Taylor, Jr., James V. Wright, and J. Norman Emerson, all of whom became leading figures in the rapidly growing field of Canadian archaeology. For years, Taylor goaded his arctic colleagues with his stimulating wit, persuasiveness, and productivity. His dissertation research (Taylor 1968) confirmed the "in-situ" model of the Pre-Dorset to Dorset transition in western Hudson Strait, and his introduction provided an excellent history of the Dorset problem. A "WET" festschrift (Morrison and Pilon 1994) produced for Taylor just before his premature death paid tribute to the career of a scholar whose scientific and administrative skills were central to the foundation of a national program of Canadian archaeology.

Concurrent with Collins' and Taylor's early work in northern Hudson Bay, Elmer Harp began surveys in 1949 and 1950 in southern Labrador and western Newfoundland. The issue here was the "affinity" of Cape Dorset culture in its extreme southeastern range, deep in subarctic territory that had a long history of Indian occupation. Harp's (1964a) analysis of the large Port au Choix Dorset site laid to rest the northeastern "forest theories" of Dorset origin that had been raised by many archaeologists of the day (Byers 1962; Collins 1962; Meldgaard 1960b, 1960c, 1962; Ritchie 1962), convincingly proved its Alaskan ancestry, and hinted at its possible ultimate Siberian origin.

Exploration in the postwar years rapidly filled in many of the lesser-known regions of the Eastern Arctic. Harp had worked in the Barrens west of Hudson Bay in 1955, testing (and refuting) the Danish Inland Eskimo origin theories. Richard ("Scotty") MacNeish (1956) spent two seasons working at British Mountain in the northern Yukon Territory searching for traces of Pleistocene human activity but mostly finding Arctic Small Tool tradition and Thule sites. Meldgaard began work at Igloodik, leading to his detailed seriation of Pre-Dorset (which he called "Sarqaa") and Dorset sites on raised beaches, and his identification of an Independence II transitional horizon between Sarqaa and

Early Dorset. Father Guy Mary-Rousselière began his work in northern Ellesmere and Baffin Islands, and in southern Baffin, Moreau S. Maxwell began his long-term study of the Lake Harbor region. Farther north, Eigil Knuth had begun in 1948 his lifelong quest of H. P. Steensby's "Musk-Ox Way" that defined the marginal and periodic 4,000-year occupations of Peary Land and other regions of North Greenland. And in West Greenland, Helge Larsen and Jørgen Meldgaard undertook work on stratified Paleoeskimo Sarqaa and Dorset sites at Sermermiut, assisted by paleoecological studies conducted by Bent Fredskild.

Many of these new finds were reported in two conferences that were instrumental in creating momentum for Eastern Arctic archaeology. The first was a series of papers delivered at the 1956 International Congress of Americanists in Copenhagen (Birket-Smith 1958). The second was a symposium held at the 25th Annual Meeting of the Society for American Archaeology in New Haven, Connecticut, that resulted in a volume titled *Prehistoric Cultural Relations between the Arctic and Temperate Zones of North America* (Campbell 1962). This conference was a watershed event that attracted most researchers working in the Northeast and Eastern Arctic. The "Dorset problem" and its southern relations were center stage, and masses of new field data were available. Despite fascination by many conferees who presented papers exploring forest ties, Giddings's new discoveries at Cape Denbigh, and the new evidence presented by Taylor, Harp, and Meldgaard all pointed to Alaska rather than to the Northeast for Dorset origins by way of its Pre-Dorset antecedent.

In addition to filling in many of the geographic gaps across the Arctic, the first series of postwar studies explored a variety of theoretical, historical, and environmental issues. During this period, internal Dorset and Thule chronologies were explored stratigraphically and the first applications of radiocarbon dating were applied. Culture history was (and continues to be) a major preoccupation of Eastern Arctic archaeology, and

the attention it received put to rest many outstanding problems, including establishing Dorset as a pre-Thule culture, as Collins (1935) had proposed earlier; affirming Pre-Dorset/Dorset ties with Denbigh and the Arctic Small Tool Tradition in Alaska (Harp 1964a); confirming a Pre-Dorset-Dorset continuum (Taylor 1968); and demonstrating relative independence of Eastern from Western Arctic prehistory. But it did not settle other issues, such as the Dorset-Norton connection or the hypothesized Thule “back-migration” to Alaska in late prehistoric times.

Theoretical issues were also explored. Although culture change discussions were still dominated by paradigms of migration and diffusion, other approaches were beginning to appear. Taylor’s Ivujivik work was the first to depart explicitly from the earlier migration perspective in favor of local development. Culture contact and diffusion were explored in the Dorset-Indian issue, and environmental change began to be seen as a potential cause or stimulus of culture change and episodic abandonment or extinction in the prehistory of Peary Land (Knuth 1952), West Greenland (Fredskild 1967; Larsen and Meldgaard 1958), and the Central Arctic (Maxwell 1960). Collins (1957) had begun to explore faunal analysis as a means of reconstructing prehistoric economy, and Harp and Knuth separately considered the impact of the environment on the survival and regional cast of peripheral Paleoeskimo cultures. Finally, archaeologists were encountering problems in the assumptions predicated by their use of “Eskimo” and “Indian” terminology to describe prehistoric groups, and Pre-Dorset and Dorset culture and their ranges became better understood and their relations to Alaskan and Northeast Asian cultures were clarified.

Expansion and Professionalism

The second half of the twentieth century witnessed an extraordinary expansion of science education throughout the Western world. Fueled in part by Cold War nationalism, by the growth of national science

agendas and funds, and by the establishment or growth of numerous academic departments, archaeological research proliferated worldwide (MacDonald 1977). Concomitant with this growth, especially as pertaining to northern research, was a shift from museum-sponsored to university-sponsored research. Interest in arctic archaeology increased in American universities, led by the University of Wisconsin, which developed a strong focus on arctic studies through the leadership of Chester Chard and William Laughlin in the 1960s and, under Chard’s direction, began to publish *Arctic Anthropology* (founded in 1964), the first journal to be dedicated to this topic. Until that time, most research had been published in *Anthropological Papers of the University of Alaska* (founded in 1952), or in museum series such as the *Bulletin of the National Museum of Canada*, *Anthropological Papers of the American Museum of Natural History*, and the interdisciplinary journal *Arctic*. With James B. Griffin’s encouragement, the University of Michigan began training the first generation of postwar Canadians who later assumed leadership roles in Canadian institutions. This growth in the discipline, with its influx of new students and ideas, transformed arctic archaeology.

The growth of academic anthropology provided archaeologists with tools to explore the prehistoric cultures of the North in ways they never could before. Radiocarbon dating, stylistic seriation, time-space systematics, cultural classification schemes, and, increasingly, interdisciplinary approaches incorporating geological, biological, climatic, and ecological modeling to understanding cultural dynamics, greatly enhanced and, some might say, embellished perceptions of the past. William E. Taylor, Jr., was fond of reminiscing that in the mid-1950s all of the practicing archaeologists in Canada could—and often did—fit into one station wagon in order to drive to the Society for American Archaeology meetings. With the growth of archaeology throughout Canada, the Canadian Archaeological Association was founded in 1967, providing its own forum for intellectual discussion.

The rapid accumulation of new field data transformed northern research. It is beyond the scope of this paper to itemize the actual advances of the 1960s and 1970s, region by region, but in general the period can be categorized as one of regional intensification that saw detailed, sustained research across the North American Arctic, for example, Harp (1964a, 1976a) in Newfoundland and Hudson Bay; Fitzhugh (1972b, 1976b), Schledermann (1971), and Tuck (1975b) in Labrador; Maxwell (1973), Marv-Rousselière (1964), and Schledermann (1975) on Baffin Island; Wright (1972a) in the Central Canadian Barrens; Taylor (1967b, 1972) and McGhee (1971, 1972b, 1974) in the Western Canadian Arctic Archipelago; Schledermann (1978a) on northern Ellesmere Island; and Knuth (1967) in northernmost Greenland. Major developments during this period were the definition of the Maritime Archaic culture in Newfoundland and Labrador as the "Eskimo-like" Indian culture of the Far Northeast; establishment of Igloodik as the *pro tem* standard for Paleoeskimo evolutionary development; a full culture-historical breakdown of Pre-Dorset, Dorset, and Thule cultures; and the recognition of strong environmental controls over cultural development, distributions, and absences. In contrast with the busy earlier decades, few publications about and little work in Greenland occurred during this period.

The Santa Fe Conference and the Core Area Concept

By 1970, archaeological practitioners, flush with a wealth of new data, struggled with organizational schemes that could systematize the accumulation of regional data sets within broader temporal and geographical contexts. In 1973, a conference titled "Eastern Arctic Prehistory: Paleoeskimo Problems" was convened at the School of American Research in Santa Fe, New Mexico, to address the integration of the archaeological record and assess the possibilities of interregional modeling and synthesis. The Santa Fe meeting was, in part, an arctic answer to the ferment instilled by the "new" or "processual" archaeology that had been

advanced during the previous decade in the United States, but which until then had made little impact on northern specialists, who were still in the preliminary stage of exploring culture history. Whereas previous arctic archaeological interpretations had borrowed heavily from ethnographic models (Schindler 1985), the Santa Fe conference (Maxwell 1976c) generated a consensus for new models of prehistoric culture change that were more explicitly archaeological.

Influenced by apparent contrasts in ecology and human settlement history between central and peripheral regions of the Canadian Arctic and Greenland, the participants came to agree on a "core area" model as a new integrative concept for Eastern Arctic prehistory (Maxwell 1976b). Combining earlier ideas of culture change by invention, migration, and diffusion with newer concepts of demography, adaptation, and ecology, they found that much of the diversity and change observed in the region could be explained by biogeographic interaction of environment and culture (Fitzhugh 1976a; Maxwell 1976a, 1976b; McGhee 1976b). Specifically, it proposed that the Central Arctic regions (the "core") in northern Hudson Bay and Hudson Strait, where food resources were relatively abundant and predictable, had archaeological records that indicated continuous occupation and gradual stylistic evolution. By contrast, areas (the "periphery") with poor or unpredictable resource abundance, such as in the High Arctic, North and East Greenland, the area west of Melville Peninsula, and southern Hudson Bay, had records indicating intermittent, short-term occupations and less gradualist culture change. Prehistory in such regions was thought to have been governed by alternating cycles of migration, florescence, and extinction, followed by abandonment, before improved conditions, usually brought about by warmer climate or resource regeneration, made a new occupation cycle possible.

The core area concept dominated the outcome of the Santa Fe conference and much of the archaeological development that followed. Combining newer and more dynamic concepts of the environment and a more

systemic approach to cultural adaptation and change, archaeological data for the first time could be marshaled into an overall explanatory framework that took regional and area-wide variation into consideration and explained culture history as a process, not, as previously viewed, as a response to vaguely defined diffusion or migration events. In addition to a more “real-time” view of linkages between culture and environment, the conference reflected such “new archeology” approaches as systems theory (Nash 1976), demography and settlement patterns (Harp 1976a), and new analytical techniques (Dekin 1976), as well as important refinements of more traditional subjects like radiocarbon dating (McGhee and Tuck 1976; see also Arundale 1981; Morrison 1989) and culture history (Fitzhugh 1976b). The conference also stimulated controversy as archaeologists of the post-Santa Fe era attempted to fit regional data into the core area model and discovered the need for refinement and accommodation of more diverse interpretive structures (Bielawski 1988; Cox 1977, 1978; Helmer 1981; Odess 1996, 1998; Plumet 1987; Schledermann 1978b).

The 1960s and 1970s also saw a more concerted attempt at environmental archaeology. Previous work, such as Steensby's geographical approach and Larsen and Meldgaard's use of Fredskild's pollen curves and climatic interpretation, did not meet modern standards of explanation by failing to identify the mechanisms by which cultures were influenced by environmental change. The “it got colder and culture changed” explanations (to paraphrase an oft-repeated Tom McGovern quip) were now recognized as explaining little. McGhee's (1969/70) paper answered this critique by proposing a specific scenario that proceeded from a demonstrated environmental event (climate warming = decreased ice cover in the Central Arctic) to an adaptive cultural change (expansion of Thule whaling eastward), which produced an observable archaeological change (Thule culture sites in the Eastern Arctic). Similar applications of cultural ecology were developed during this period for culture change in Peary Land (Knuth

1967), Labrador (Engstrom et al. 1984; Fitzhugh 1972b; Fitzhugh and Lamb 1985; Foster 1983a, 1983b; Jordan 1975a, 1975b; Short 1978), Hudson Bay (Fitzhugh 1976a), Baffin Island (Schledermann 1975), and Greenland (Meldgaard 1977; Petersen 1974-1975). Some of these models were later challenged for being too environmentally deterministic (Schindler 1985), for lacking sufficient environmental data, or for failing to link environment and culture in a definable way; but they, nevertheless, advanced understanding beyond the level of earlier migration models.

Surveys and Environmental Archaeology

Another category of research that developed during this period grew out of intensive survey programs that sought to develop more detailed site inventories and land-use patterns. Rather than taking the site as the focus of archaeological work as most earlier projects had done, settlement pattern archaeology was directed at cultural behavior in landscape units of varying sizes and across ecological or geographical boundaries. This work attempted to reconstruct cultural adaptation types and seasonal movements throughout an annual subsistence cycle to better understand cultural patterns and variability—variability in residence type, economy, technology, and assemblage—on a synchronic horizon. Building from smaller to larger land-use units (band, multiband, inter-ethnic territories), and crossing ecological borders, allowed this approach to integrate culture histories at larger scales and seek data on the interaction of whole cultures with changing environments through time and space. Steensby's (1917) first steps in this direction had been expanded in the 1930s by Danish researchers who mapped the Norse farms and churches in Greenland. In Canada, the use of surveys as a method for finding sites had begun with the Fifth Thule Expedition of 1921–1924 (Mathiassen 1927b) and were employed by William J. Wintemberg (1939, 1940) and Elmer Harp (1951) in Newfoundland and by the latter in Keewatin (Harp 1958), as well as by Eigil Knuth (1967) in Peary Land.

But integration with environmental studies and cultural ecology did not begin to play a role in regional approaches until Harp began his survey and aerial photography project on the east coast of Hudson Bay and the Belcher Islands (Harp 1976b). Building on Harp's model, William Fitzhugh (1972b, 1977b, 1980b) applied environmental analysis and broad-scale survey techniques to the culture history of the forest-tundra boundary in Labrador and, together with his associates (Hood 1981a; Jordan 1975a, 1975b; Kaplan 1983; Loring 1992; Nagle 1984), developed an integrated view of Eskimo and Indian culture change and environmental interactions. Less environmental but equally broad in regional scope were Patrick Plumet's surveys of the New Quebec coast and northern Labrador, reported extensively in a new monograph series he established called *Paléo-Québec*.

An important contribution to survey methods was made when archaeologists began to collaborate with geologists to develop land-emergence curves based on radiocarbon-dated shells, whale bones, and sometimes archaeological site samples (Andrews et al. 1971; Plumet 1974). A long-term program of geological and archaeological dating in Labrador eventually led to the construction of detailed marine limit and uplift curves for the entire Labrador coast (Clark and Fitzhugh 1991; Fitzhugh 1972b, 1973). In addition to informing on settlement pattern changes and paleogeography, these data led to the discovery that glacial ice had persisted on the central coast into Early Maritime Archaic times (Clark and Fitzhugh 1990).

At the same time, and continuing into the 1990s, regional surveys expanded rapidly throughout the Eastern Arctic and Greenland as transport facilities and support improved, aided in Canada especially by helicopter hours provided by the Polar Continental Shelf Project. The most extensive survey in the Central Canadian Arctic was organized by Allen McCartney and James Savelle in the early 1980s to inventory early Thule whale-bone-bearing sites, partly as a response to site damage by contemporary Inuit removing whale bone and partly

to learn about early Thule settlement patterns, whale-hunting strategies, and the Thule impact on bowhead populations (McCartney and Savelle 1993).

Historical Archaeology and European-Inuit Contact Studies

The archaeological fixation with origins research has long placed a premium on fieldwork that focused on distant prehistory at the expense of studies at less distant times. However, in Greenland, Danish archaeologists like Daniel Bruun have been mapping and excavating Norse sites since the 1890s. More recently, Viking age settlements have been the focus of broad historical, environmental, and economic reconstructions from a total North Atlantic perspective (Bigelow 1991; McGhee 1984a; McGovern 1980, 1981a, 1981b, 1990). The discovery of the L'Anse aux Meadows Norse site in northern Newfoundland, the probable "Vinland" of Leif Eriksson (Ingstad 1977; Ingstad 1969; Wallace 1991, 2000a, 2000b), has done much to put archaeological flesh on the alluring bare bones of Norse sagas.

While great progress was made in Paleoeskimo research, the archaeology of the historical era in the North, both of European sites and of Inuit sites at contact, has lagged behind prehistoric archaeology and only recently has gained the attention it deserves. Relatively few post-contact Inuit sites in the Arctic have been excavated scientifically despite the fact that many of them have been of great historical interest to latter-day explorers, tourists, historians, and anthropologists. Several projects utilized historical (i.e., European) material culture as an integral part of their studies (e.g., Fitzhugh 1985a; Jordan 1978; Jordan and Kaplan 1980; Kaplan 1983, 1985), but it is only recently that research directed specifically on the archaeology of Inuit-European contact has emerged, in Greenland, Frobisher Bay, and Labrador (Alsford 1993; Auger 1991, 1993; Auger et al. 1993; Cabak 1991; Fitzhugh and Olin 1993; Gullason et al. 1993; Gulløv and Kapel 1979; Henshaw 1995; Loring 1998). In Newfoundland and southern Labrador, Selma Barkham's (1980) historical research

led to the archaeology of the previously unknown Basque stations and shipwrecks in Red Bay (Tuck 1982; Tuck and Grenier 1989). In part, research on the recent past has responded to the needs and interests of native communities that find the traces of their immediate ancestors more interesting than those of ancient prehistoric inhabitants of the Arctic, while the growth of historical archaeology has resulted from public interest in early European settlement and the economic opportunities of tourism and development. These studies have been gradually breaking down the academic barriers that have existed between historical archaeology (the archaeology of Europeans) and prehistory (the archaeology of indigenous people "without history"). There is still much fascinating research to be done on European-Native contact archaeology throughout the region, both at Inuit and at European exploration, whaling, and trading sites. Parks Canada has made significant progress in this direction through the preservation and excavation of some of the most important European sites; and the Meta Incognita Project has promoted research on the history and archaeology of the Martin Frobisher voyages (Symons 2000).

Government Programs, Funding, and Resource Management

Another feature of the past fifty years that deserves notice even though it is not identified in the following papers is the changing nature of archaeological funding and the growth of government archaeology programs. Before World War II, most funding for archaeological research in the North was based on private sponsorship by museums, individuals, or corporate groups, including newspapers, businesses, and research foundations like the National Geographic Society, the Royal Geographical Society, Carlsberg Foundation, and others. Government in some cases provided transportation and use of local facilities like the RCMP or the navy, but this assistance was relatively limited. Probably the most important infrastructure used by archaeologists in the Canadian North was the Hudson's Bay

Company, whose posts scattered throughout the region provided assistance to scientific teams as local "hostels," entrees into the local community, and offered both indirect and direct subsidies to field programs. In Greenland, a similar pattern prevailed. There, the outstanding example of local support was Knud Rasmussen's post at Qannaq, whose commercial trade profits allowed it to fund part of Rasmussen's Fifth Thule expedition. In the 1940s, missions and RCMP stations expanded the facilities and social networks available to archaeologists in many of the most inaccessible areas of the North.

After the war, the growth of a southern-based infrastructure in the North added a new source of support for field archaeology. Funding by the Office of Naval Research provided new resources for science in the North American Arctic, and by the 1960s the creation of peer-reviewed national science councils and research foundations in Canada and the United States made it possible to support a great increase in non-museum-based university research. Later, in Canada, support from oil and gas exploration through the Polar Continental Shelf Project provided invaluable assistance in logistics and transportation. The rapid growth of village-based government programs and facilities throughout the Canadian North after 1960 brought another source of resources into the reach of perennially cash-starved arctic research projects.

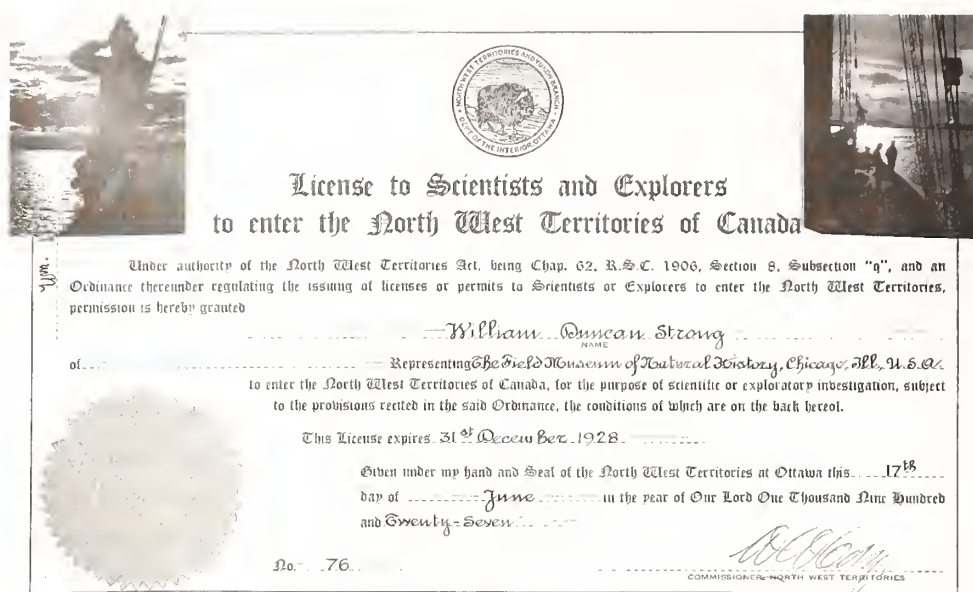
Growth in governmental programs explicitly devoted to supporting archaeological collecting and research also occurred in the south. Before the 1960s, archaeology was conducted in the North by researchers and museum scientists who operated largely independently of controlling authorities outside their home institutions. Although Canada has had legislation protecting archaeological materials in the Arctic since 1926 (fig. 1.4), actual oversight and management of field research and archaeological collections did not become feasible until the National Museums of Canada (later the Canadian Museum of Civilization) became a central authority in the permitting process in the 1960s.

Since the predominant territory of the Eastern Arctic fell within the Northwest Territories, Ottawa was the nexus for administering archaeological initiatives throughout the region and the depository for the collections at the Archaeological Survey of Canada (ASC).

The latter organization was created at the National Muse-

ums of Canada in the early 1970s, in part to strengthen its funding base and its national leadership role in archaeological research and resource management. These developments also led to a national registry of archaeological sites and collections. While these changes consolidated much of the control over arctic research within the Archaeological Survey of Canada, developments within the provinces having arctic lands, especially in Quebec and Newfoundland-Labrador, and more recently the Northwest Territories/Nunavut, led to agreements with Ottawa that resulted in their assuming regional management authority for their arctic materials. Today, conservation has been added as an important management function, and decentralization has strengthened regional authorities like the Prince of Wales Northern Heritage Center. During this period, development of a community response process has given local communities important input into the permitting process.

A parallel process has developed under the aegis of Parks Canada to support archaeological and historical research on sites within the growing jurisdiction of this agency. Originally mandated for major southern sites like Louisburg and other nationally registered



1.4/ William Duncan Strong's permit to conduct scientific research in the North West Territories, Canada, 1927 to which Strong attached photographs of himself and the Bowdoin in Frobisher Bay.

historic properties, Parks Canada's involvement in arctic landmark sites, historical places, and national parks has resulted in a steady growth of its role in arctic archaeology, and one that is certain to become more important in this age of shipborne and adventure tourism. While Parks Canada's interests are site-specific rather than regional and its priorities are based more on management and preservation than on research, its increasing support for research, conservation, publication, and public information is becoming a major force in support of arctic archaeology.

As archaeology became both more popular, and sometimes more controversial, from the local perspective, government expanded its archaeological programs beyond the mere permitting process and began to make research results available to the public. The establishment of the Archaeological Survey of Canada monograph series at the National Museum of Man in 1972 has provided a crucial mechanism for publication of major works. The annual publication series, *Archaeological Research in Newfoundland and Labrador*, beginning in 1980, provided annual research updates for this province; the Quebec Government Service du Patrimoine began to issue periodic reports and bibli-

ographies (Martijn 1998); the Danes began to provide annual abstracts in the 1980s and, with the establishment of the Danish Polar Center, have recently upgraded them to full-scale reports (Arneborg and Gulløv 1998); and the Prince of Wales Northern Heritage Centre began publishing illustrated abstracts of archaeological projects for popular consumption in the early 1990s. The past decade has also seen important institutional developments facilitating archaeological work in the Eastern Arctic, including the creation of the Arctic Studies Center at the Smithsonian's National Museum of Natural History in 1988, the Danish Polar Center in 1989, and the Greenland Research Center at the Danish National Museum in 2000.

International Research and Collaboration

For the most part, Eastern Arctic archaeology has benefitted from international cooperation and access, with a minimum of politics and acrimony. Undoubtedly, the basis for this situation resides in political history, especially that of the Canadian North, which has been more open to international research than has the Danish sector.

The "open" nature of the Canadian Arctic resulted as much from a long history of British, Scandinavian, and American exploration as from its commercial history in which the Hudson's Bay Company (HBC), whaling, and mining interests provided many routes of access for English-speaking scholars (e.g., Hall's discovery of the Frobisher sites came as a result of assistance provided by the New Bedford whaling community; Stefansson's arctic explorations in 1914-1917 were facilitated by the HBC and other commercial parties, as was the Danish Fifth Thule Expedition that followed in 1921-1924). After the Fifth Thule Expedition, which brought Scandinavians into the Canadian Arctic and Alaska, for the most part Nordic scholars gravitated to Greenland while British, Canadian, and American scholars took up work in Canada. In the 1960s, however, Jørgen Meldgaard, a Dane, excavated at Igloodik; Anne-Stine and Helge Ingstad, Norwegians, discovered and

excavated for six years at the L'Anse aux Meadows Norse site in Newfoundland; and in the 1970s, Thomas McGovern participated in Nordic projects at Norse sites in Greenland. American involvement in Canadian arctic and subarctic archaeology has been extensive, beginning with Henry Collins's work on Southampton Island, Frobisher Bay, and Resolute; Elmer Harp in the Barrens, Newfoundland, Labrador, and Hudson Bay; Allen McCartney in Hudson Bay and the Central Arctic; William Fitzhugh, Richard Jordan, Steven Cox, Susan Kaplan, Christopher Nagle, and Stephen Loring in Labrador; and Fitzhugh, Anne Henshaw, and Daniel Odess in Frobisher Bay, to mention only a few. Most of these projects also included Canadian students and professionals.

The more recent history of international work in the Eastern Arctic is alluded to in some of the papers presented in this volume. The impacts of attempts to restrict access to foreign archaeological projects, applied first by the Danes in Greenland and later, more selectively, by Canadians in the Canadian Arctic, were justified, to varying degrees, as a way to help preserve archaeological resources for exploration by home nationals. Far more effective in developing nationality-based programs of arctic archaeology have been the hiring practices of American and Canadian universities and museums in all of the arctic-interest countries and the pressures applied on foreign researchers indirectly and directly through funding and transport allocations, access to students, and support of local community organizations. Two American projects, in particular, could provide interesting case studies in Canadian archaeological politics: Allen McCartney's and James Savelle's (McCartney 1979; McCartney and Savelle 1993) Thule Whalebone Conservation Project and the Archaeology of the Frobisher Voyages Project conducted by the Smithsonian's Arctic Studies Center between 1990 and 1993 in collaboration with the Canadian Meta Incognita Project (Alsford 1993; Fitzhugh and Olin 1993; Fitzhugh 1997). Both had significant Canadian participation in their scientific teams, and both experienced political difficulties.

Although brief mention has already been made of new ecological, climatic, and environment research approaches, techniques of microanalysis have contributed much to the development of the field in the past three decades. Advances in archaeometric techniques have made it possible to identify ("fingerprint") and trace lithic materials by petrographic, age, and /or chemical signatures from site to quarry (or region), allowing archaeologists to reconstruct prehistoric cultural influence, trade, exchange, and migration. To date, these techniques have been applied primarily in Labrador and northern Quebec to quartzites, cherts, soapstone, and nephrite (Allen et al. 1978, 1984; Archambault 1981; Boutray 1981; Fitzhugh 1972b; Gramly 1978; Lazenby 1980; Nagle 1984, 1986; Plumet 1977, 1979c, 1981b; Rogers et al. 1983), but such studies will be equally valuable for similar interpretations throughout the Eastern Arctic. Some institutions, like the Archaeological Survey of Canada, have established lithic resource type collections. Studies of iron, copper, and bronze have been of particular use in distinguishing native metals from those introduced from Asia or by the Norse (Buchwald and Mosdal 1985; Harp 1974–1975; McCartney and Mack 1973; Schledermann 1980). Similarly, research on cloth, yarn, and cordage from frozen Norse sites in Greenland has resulted in the identification not only of fibers of domestic sheep, goat, and oxen, but also of caribou, polar bear, arctic fox, arctic hare, and possibly musk-ox, as well as distant contributors like brown bear and bison, suggesting contact and perhaps trade between Norse and Dorset or Norse and Thule peoples in Canada (Berglund 1998, 2000). Musk-ox hair twine and rope were in use among Dorset people in the Eastern Arctic (Andrews et al. 1980; Jordan 1980) and the presence of Norse yarn, fabrics, bronze objects, and European wood in Dorset and Thule sites in Baffin and Ellesmere raise further the possibility of more extensive interaction between the Norse and these Canadian peoples (Schledermann 1980; Sutherland 2000).

Archaeozoological applications have also contributed much to the understanding of prehistoric diet, economy, environmental adaptation, and seasonality, and are universally conducted now as part of the standard suite of analytical techniques (e.g., Cox and Spiess 1980; Grønnow et al. 1983; Henshaw 1995, 1999; McGovern 1980; McGovern et al. 1983; McGovern et al. 1996; Møhl 1972; Spiess 1978; Stenton and Park 1994; Woollett 1999). Paleoecological studies of insect microfauna have provided information about Viking migration, domestic living conditions, and climate change in Greenland and the North Atlantic (Buckland 2000), and identification of archaeological wood and charcoal has produced information on regional environmental history (Fitzhugh 1978a), ocean currents and climatic history seen through the lens of driftwood (Eggertsson and Laeyendecker 1995), and imported Elizabethan technology at the Frobisher Baffin Island sites (Laeyendecker 1993a, 1993b). Most recently, interdisciplinary research that has become a hallmark of Viking studies around the North Atlantic rim coordinated by the North Atlantic Biocultural Organisation (NABO) (McGovern et al. 1988; Ogilvie and McGovern 2000) has returned for another phase of research exploring the complex forcing mechanisms between climate, environment, and culture in Labrador Inuit cultural development (Kaplan and Woollett 2000). Similar studies are now being conducted in western and northern Newfoundland by Priscilla Renouf, Trevor Bell, and Joyce MacPhearson.

On a more humanistic note, researchers have explored symbolic meaning expressed by the dichotomy of terrestrial and marine worlds seen in the segregated use of ivory and antler artifacts associated with land and sea hunting (McGhee 1977), while others have explored Maritime Archaic spiritual ties to landscape expressed in engraved soapstone pendants (Fitzhugh 1985c), or have explored the mystical mind of Dorset people or the sexual symbolism of Thule material culture as seen in their art, clothing, and shamanistic implements (Jordan 1979–80; McGhee 1974–75; Meldgaard

1960a; Sproull-Thomson and Thomson 1981; Sutherland 1997; Swinton 1967; Taylor 1967a; Thomson 1982). Even ideas about handedness and the possible impact of European diseases on the demise of Dorset and Thule people have been tentatively explored (McGhee 1994). And although gender has not been the explicit focus of prehistoric study, it forms the core of such investigations as Cabak's (1991) research on the interactions between Moravian missionaries and the adoption of village life by Inuit in Labrador and Gullason's (1999) exploration of 500 years of Thule-Inuit culture in Frobisher Bay. Finally, the meticulous ethnolinguistic and photographic documentation seen in Norman Hallendy's (2000) *Inuksuit: Silent Messengers of the Arctic* established the first treatment of symbolic cultural landscapes to be explored in the circumpolar region.

Synthesis and Popular Prehistory

One of the important outgrowths of the intensive research of the 1960s–1980s was the publication of several book-length archaeological syntheses. Following several early overviews by Henry Collins and Diamond Jenness, the first comprehensive treatments by Hans-Georg Bandi (1969) and Don Dumond (1977) covered the entire North American and Greenland Arctic. Eigil Knuth (1967) produced a synthesis of Peary Land archaeology in 1967. In 1977, Jørgen Meldgaard prepared a regional synthesis for Greenland and William Fitzhugh produced a regional overview of Labrador, and in 1978 Robert McGhee published a popularized treatment, *Canadian Arctic Prehistory*. That same year, James A. Tuck published a similar overview, *Newfoundland and Labrador Prehistory*, and a year later Volume 5 of the Smithsonian Institution's *Handbook of North American Indians* (Damas 1984) appeared with overviews and specific treatments on Canadian Arctic and Greenland archaeology (Fitzhugh 1984a; Jordan 1984; Maxwell 1984; McGhee 1984b). However, it was not until 1985 that a truly comprehensive Eastern Arctic study appeared, *Prehistory of the Eastern Arctic*

(Maxwell 1985), providing researchers, students, and the public for the first time with a detailed overview, including excellent photographs, illustrations, and maps, of the Eastern Arctic's 4,000-year culture history. More recently, McGhee's (1996) *Ancient People of the Arctic* updated the Bandi and Dumond overviews, and two symposia synthesized information on Paleo-eskimo cultures (Grønnow and Pind 1996; Memorial University 1986). Also during this period archaeology in Nunavik (Nouveau-Québec) advanced rapidly, following the work of Taylor and Plumet and the institution of journals like *Recherches Amérindiennes au Québec* (since 1971) and *Études Inuit Studies* (since 1977).

Following in the personalized tradition of Louis Giddings's *Ancient Men of the Arctic* is Peter Schledermann's (1996) *Voices in Stone*, an account of his work in Ellesmere Island. Also emerging from the "Ancient Men" tradition were other more popular works. Robert McGhee's (1976a) booklet *The Burial at L'Anse-Amour* about a 7,500-year-old Maritime Archaic burial in southern Labrador was the first dramatized reconstruction to put flesh on the stones and bones of northern Canadian prehistory. This was followed by *Thule Pioneers* (Bielawski et al. 1986), an overview of Thule culture produced for northern residents, and Priscilla Renoufs's (1999) book *Ancient Cultures, Bountiful Seas: The Story of Port au Choix*, a popular account of the archaeology of Port au Choix, Newfoundland. Peter Schledermann's (2000) recent novel, *Raven's Saga*, reconstructs Norse and Inuit cultures and contacts in northern Greenland and Ellesmere. The appearance of popular series produced for (and more recently by) Inuit, such as *Inuktitut*, issued by the Department of Indian and Northern Affairs in Ottawa, and *Tumivut* ("our footsteps"), published by the Avataq Cultural Institute in Quebec, makes information on Inuit culture, history, literature, art, and archaeology available to a general northern audience.

Finally, as the elders' generation has matured, festschrift volumes have appeared for William E. Taylor (Morrison and Pilon 1994), Eigil Knuth (Grønnow and

Pind 1996), Jørgen Meldgaard (Gilberg and Gulløv 1997), and Elmer Harp, Jr. (this volume), offering new perspectives on Eastern Arctic archaeology and its history.

Emergence of Community Archaeology

While arctic archaeologists have always revelled in the glory of the northern landscape, with its inherent adventure and its freedom for vistas and imagination, still the most compelling aspect of fieldwork is the opportunity provided to meet and travel with the people whose homeland it is. There is a long history of northern native colleagues working with archaeologists, as guides, provisioners, crew members, caretakers, and informants. As resourceful as many archaeologists believed themselves to be, there was always the knowledge that should supplies run low, logistics be disrupted, travel routes confused, or knowledge about resources, weather, or the terrain be desired, local community members could, and often did, provide critical insight and help. In this respect, and much more, northern native peoples have always figured significantly in Eastern Arctic research.

In retrospect, it was presumptuous for archaeologists from the south to have hoped to really understand the world of northern hunters. Perhaps this might never have been possible, and certainly not without the insight and knowledge of the people who reside there. Perhaps more so than anywhere else in the world, northern archaeology is still closely linked with similar lifestyles, subsistence patterns, and behavioral strategies to those that existed in the past. Northern archaeology has always retained a strong ethnographic bias, derived in part from Inuit insight and knowledge. Yet archaeology has remained primarily an exercise devised by, and created for consumption by, audiences far to the south of the localities from which this knowledge was extracted. With this fact in mind, it seems appropriate to question the motives of archaeology as they might be perceived by northern natives: Is archaeology only another expression of Western "imperialistic" tendencies? Having gained control over land

and resources, is Western society now extending its hegemony over the past as well? Perhaps more reflective in its middle age, arctic archaeology has in the past decade or so re-dedicated itself to the challenge of creating a past that has interest and meaning for Inuit consumers as well as for the traditional outlets of scholarship. Archaeologists, however, have been slow to meet this challenge (Cirouard 1977; Swinton 1977).

Arguably, the most significant development in arctic research since Dekin's review has been the growth of what is generally now called "community archaeology" (Loring 2001; Nicholas and Andrews 1997; S. Rowley, this volume; Stenton and Rigby 1995). Community archaeology is committed to addressing community agendas and needs. It combines the expertise and training of archaeologists with the insight and knowledge of community leaders and educators in order to provide experiences and training for Inuit young people. The participation of and reliance on Inuit elders is a critical component of such endeavors. The elders provide interpretations of the archaeological features and assemblages; the knowledge and beliefs they convey in interviews and stories reaffirm Inuit community values and testify to the validity and significance of Inuit knowledge. Frequently, cultural, historical, and community values are strengthened as a consequence of such projects. In addition to the goals of traditional archaeology, which mandate primarily the growth of knowledge through publications and archiving of collections, community archaeology projects typically result in local exhibits, school programs, and, in some cases, the resulting collections remain in the North.

To the extent that community interests and initiatives define the need for research, many aspects of community archaeology were anticipated by the Thule Archaeology Conservation Project begun by Allen McCartney and James Savelle in 1976, which sought to meet the needs of Inuit artisans for archaeological whale bone (McCartney 1979). The project trained Inuit crew members and was instrumental in instilling a sense of

concern for the preservation of historic structures at a community level. From a scientific point of view, the project has paid extraordinary dividends, for it marked the inception of an international interdisciplinary research regime now more than twenty years old (McCartney and Savelle 1993). At about the same time, in Greenland, archaeologists experimented with the possibilities of including archaeological experiences as part of the program at summer youth camps that saw heritage projects as a means of reaffirming traditional community values.

From a historical perspective, archaeological research has been used to support specific community needs. Demonstrating past land use has been a critical feature of land-claim negotiations in the Arctic, and archaeologists have participated in the past by compiling land-use and cultural chronology position papers in support of land claim litigation (Fitzhugh 1977a; Jordan 1977; McGhee et al. 1976). But while this research was in support of community mandates, it was presented in a format and structure characteristic of professional archaeologists.

The continuity between the past and the present in the North makes archaeology a logical extension of ethnography and, perhaps not surprisingly, has spawned its own discrete literature (e.g., Janes 1983). Oral history research that addressed such questions as traditional land-use, settlement-subsistence strategies, the nature of camp structures and activities, and the role of exchange in social life are obviously of significance to archaeology. Examples of archaeological research that have specifically incorporated oral history into fieldwork methodology include a project with the Inuvialuit on Herschel Island and the Yukon North Slope (Nagy 1994b), surveys by the Arviat Historical Society (Henderson 1997), and Webster's (1994) Piquiq Research Project with Baker Lake elders at traditional caribou crossing places on the Kazan River, among others. The Baker Lake Historic Sites Project in 1983 (Kabloonaa 1984; Stewart et al. 2000) was a community initiative to develop the tourist potential of old

Thule ruins. These projects all combined professional archaeologists working with community elders and native interns to document and understand traditional sites. Coinciding with the increasing sensitivity of archaeologists to Inuit concerns about research has been a pronounced commitment to employing Inuit students in fieldwork. Native organizations like the Avataq Cultural Institute, the Attikamek-Montagnais Council, and the Cree Regional Authority, among others, helped facilitate student participation in prehistoric research conducted within their jurisdiction and associated with large-scale resource development.

It is this climate of increased Inuit participation and interest that led a number of archaeologists to instigate fieldwork that specifically sought to address local interests in archaeology by providing training and fieldwork experience for young people. In the last decade, community archaeology projects have spread across Canada from the N.W.T. (Bielawski 1984) to Labrador, with programs among Labrador Inuit (Cabak 1991; Loring and Baikie 1992) and Innu (Loring and Ashini 2000). Two programs deserve special recognition for their sustained commitment to community archaeology: Susan Rowley's Ataguttaaluk Field School in Igloolik (S. Rowley 1991, this volume) and the field school and training opportunities provided by the staff of Arctic College in Iqaluit (Stenton and Rigby 1995). With the increased opportunities for research and fieldwork and through the implementation of training programs designed for northern native students (Bertulli 1985; Suluk 1994; Webster 1985), it is clear that the future of archaeology in the Eastern Arctic will include more northerners and northern interests.

The potential revolutionary impact of such developments on the practice and perception of the Inuit past could be startling. For one thing, native archaeologists would bring language skills and an awareness of community values to the practice of archaeology. A native archaeology would have considerably freer access to community elders with their erudition and wisdom, their so-called traditional ecological—or

indigenous—knowledge (Berkes 1993; Bielawski 1996; Brooke 1993; Cruikshank 1981, 1984; Freeman and Carbyn 1988; Saunders 1992; Stevenson 1996). Such intimate knowledge of the distribution and availability of resources, as well as of arctic ecology, and a historical perception of the relationships between human beings and their environments, will go far to humanize the wilderness of arctic prehistory. An Inuit prehistory that includes place-names and stories (e.g., Hallendy, this volume) can make the past more meaningful and more accessible to the communities whose legacy it is than a past that is derived exclusively from radiocarbon dates and an arcane academic synonymy. With such an increased political awareness and agenda, the future of arctic archaeology will be a collaborative venture probably quite different from what could have been imagined by the elders in the archaeological community, but one that all will agree can only be richer for the sharing of insights and diversity of visions.

The Ittarnisilirijit Conference

Stimulated by their involvement in the Dartmouth Elders Conference in 1993, Inuit participants Gary Baikie, Deborah Webster, and George Qulaut organized their own conference at Igloolik in February 1994, with the express purpose of providing a forum for Inuit to discuss the significance and practice of archaeology in their communities (Bennett 1994; Phillips 1994; Webster and Bennett 1997). The *Ittarnisilirijit* (“those who deal with the distant past, the time of legends”) Conference brought together delegates from across the Canadian Arctic, including Inuit elders, young people, and members of Inuit communities with an interest in cultural heritage programs and archaeology. A small group of archaeologists who had pioneered a community-oriented approach (Paul Antone, Bjarne Grønnow, and Susan Rowley) were also invited. Northern television, radio, and print media covered the conference, which had as its goals “to provide an opportunity for Inuit archaeology and history specialists from across the north to meet and exchange information, and the other

was to produce a list of guidelines for archaeological work in the Inuit homeland” (Bennett 1994:2). Delegates ran the gamut from those calling for a total cessation of archaeological research to those maintaining that there was a place for archaeology in Inuit communities. It was felt that because much traditional culture had already been lost, archaeology provided a means by which young people could learn more about their heritage and history. The conference report concluded:

As delegates learned more about each other's experiences over the course of the conference it became clear that Inuit can benefit from archaeology when they participate in it and have control over how it is practiced in their land. Young people learn new skills and gain deeper understanding of their own culture; elders have the satisfaction of passing on their knowledge to young people; and when the results of the research are shared with the people of the local community, they have the opportunity to learn more about their own history. When the community works in partnership with archaeologists on a project from beginning to end and Inuit expertise is used, the quality of the archaeological research improves. This benefits everyone. (Bennett 1994:3)

Of foremost concern to the conference participants was to devise a means by which Inuit would be empowered as full participants in the process of archaeology. The conference concluded with the formation of a set of guidelines to govern future archaeology on Inuit lands. Over and over again conference delegates expressed concern that the knowledge of their past was a commodity that must be shared, and that future research initiatives should include video and other media documentation that would bring archaeological insight and results to local community members.

The Dartmouth Elders Conference

A sign of a maturing discipline is its concern with its origins and development. It was in light of these concerns that an academic gathering titled “The Elders Conference: The History and Practice of Eastern Arctic Archaeology” was convened at Dartmouth College in

Hanover, New Hampshire, on April 22–24, 1993. The conference sought to review the wealth of accumulated research results from the Eastern Arctic; to honor a large, vigorous, articulate group of “elder statesmen”; and to assess and evaluate progress and goals in the face of rapid changes in modern social, political, and personal values. The timing was important in the celebratory sense of our archaeological elders, several of whom were passing their eightieth birthdays (and Knuth his ninetieth) in that year. But it was also important because archaeology in the region had reached a major point of change in orientation and goals. It was, therefore, time to take stock, to understand the historical development of the field, and to chart new directions for the future.

In contemporary Inuit communities, recognition and celebration of the importance of community elders places a premium on respect for traditional values and knowledge while promoting a resurgence of pride in Inuit identity. Even the Inuit now living semi-sedentary lives in relatively permanent villages still afford elders a special status. As the repositories of community history they know the social obligations and intricacies that define group identity; their lifetime accumulation of knowledge and experience is critical for scheduling settlement and subsistence decisions; and their memory provides access to landscapes of myth and ritual. In emulating this tradition, the Elders Conference sought to honor and celebrate the mentors and leaders of the practice of archaeology in the Eastern Arctic.

Motivation to hold the conference at Dartmouth College was due in part to recognize and celebrate the accomplishments of Elmer Harp, Jr., on the occasion of his eightieth birthday. His career from the 1940s to the 1990s spans the years during which Eastern Arctic and Subarctic archaeology developed from a pioneering field into a fully professionalized enterprise, in which he played a central role and worked in a wide variety of regions, from Newfoundland to Keewatin. Equally important has been his role in teaching anthropology and in training and introducing students to the

North, a task in which he was ably assisted by his ethnology colleague at Dartmouth, Robert McKennan.

As plans for the conference progressed, the organizers discovered that two other pioneers of Eastern Arctic archaeology, Graham Rowley and Father Mary-Rousselière, were also celebrating their eightieth birthdays in this year. A convocation devoted to honoring their accomplishments proved to be a perfect magnet for bringing together an extraordinary group of their colleagues—pioneers themselves—including Frederica de Laguna, Edmund Carpenter, William S. Laughlin, William E. Taylor, Jr., Jørgen Meldgaard, James V. Wright, and others. The presence of these “elders,” who were trained or influenced by the founders of Eastern Arctic archaeology, Diamond Jenness, Henry Collins, and Therkel Mathiassen, provided a perfect opportunity to convene a conference addressing concerns with the history and practice of Eastern Arctic archaeology.

After a welcoming reception on Thursday evening, the conference began on Friday morning at the Hanover Inn. The first session, “Speaking of Elders,” afforded the senior participants an opportunity to address the group. Frederica de Laguna, Graham Rowley, Elmer Harp, Father Guy Mary-Rousselière, and Edmund Carpenter reflected on their inspirations and revelations of fieldwork. Until nearly the last minute Eigil Knuth had hoped to attend, but his planning for an early summer departure for Greenland conflicted. He did send a telegram hoping to see everyone “after my return from Peary Land.” Two afternoon sessions focused on Greenland and on the Central Arctic. In the first, Jørgen Meldgaard, William Laughlin, Hans Christian Gulløv, Bjarne Grønnow, and Morten Meldgaard presented papers; in the second, Robert McGhee, Bryan Gordon, John Cook, Patricia Sutherland, and George Wenzel spoke.

A banquet Friday evening offered an opportunity to honor Elmer Harp. Jørgen Meldgaard commenced the festivities with a toast in which he reminisced about Harp’s visit to Copenhagen and a lecture that Elmer had presented at the old lecture hall of the Royal

Nordic Society for Antiquities at the Danish National Museum. Meldgaard allowed as how it was “a distinguished performance” and how thereafter Harp was universally referred to as “the Gentleman from New England.” But, as Meldgaard impishly noted, he was not the first to have received this accolade, for in 1831 another “young gentleman” from New England—Henry Wadsworth Longfellow—had traveled to Copenhagen to meet Professor Carl Rafn, editor of *Antiquitates Americanae*. Rafn had an abiding interest in the Norse presence in North America, a belief he felt was confirmed by the discovery of a skeleton covered with copper “armor” in a Fall River, Massachusetts, grave in 1831 (Wallace and Fitzhugh 2000; Willoughby 1935: 232–234). While the skeleton subsequently proved to be that of a Native American, the specter of a Norseman in New England resulted in Longfellow’s poem “The Skeleton in Armor.” It remained for Elmer Harp, with his crew’s recovery of a pendant of smelted eleventh-century Norse copper in a Dorset site at Richmond Gulf on the east coast of Hudson Bay (Harp 1974–1975), to prove the poet’s fancy.

Tributes followed to Elmer as a teacher (Fitzhugh) and as a photographer (Renouf). Elmer’s photographs of the community at Port aux Choix are a testament to the underlying humanist paradigm in anthropology that archaeology, at its best, is heir to. The close links between these fields were further elaborated upon by tributes to Diamond Jenness by David Morrison and by Susan Kaplan to her recently deceased professor and colleague, Richard Jordan, who, like many others, had begun his career as a Dartmouth student inspired by Robert McKennan and Elmer Harp.

Saturday morning was devoted to papers on the Quebec-Labrador peninsula, where Harp’s work was principally centered. Bryan Hood, Albert Dekin and John Kilmarx, Stuart Brown, Jane Sproull-Thomson and Callum Thomson, Christopher Nagle, Patrick Plumet, Charles Martijn and Jean-Yves Pinal, Moira McCaffrey, and Ian Badgley presented views on the history of archaeology in this region. The connections and relationships

between the Northeast and the Arctic that framed much of the early research in this region was discussed in papers by Stephen Loring, Bruce Bourque, and Steven Cox. Then, having discussed the past, the conference turned towards the present with papers by Norman Hallendy, James Helmer, and Susan Rowley that stressed the significance of research conducted in collaboration with Inuit elders.

Discussion and debate led to stories and reminiscences as Saturday afternoon gave way to evening. On Sunday morning, discussions continued, ending with a presentation by the three Inuit participants at the conference, George Qulaut (Igloolik Research Laboratory), Deborah Webster (Parks Canada, Yellowknife), and Gary Baikie (Torngasok Cultural Center, Nain) reflecting on their participation in the conference and the future of the past in the Canadian Arctic (fig. 1.5). Their joint statement presented to the gathering seems an appropriate ending to this paper:

Inuit respect their elders and we, Inuit, would like to pay respect to your elders today. Last night we talked about how far archaeology has come in our lifetime. We were also discussing how much further it has to, and will, evolve. Thanks to a lot of you, Inuit are starting to work “with” archaeologists instead of “for” archaeologists. We are also starting to study archaeology at various colleges and universities. Inuit high school students are becoming more involved and interested in archaeology.

Inuit communities are becoming more involved and more outspoken. Inuit are asking more questions about archaeological work being conducted in their area and are wondering what happens to the artifacts that leave with the archaeologists after the field season.

People tend to answer these questions themselves, and since they do not have all the information at hand they may come up with wrong answers. This may evolve into mistrust towards archaeologists by the people. This mistrust can be alleviated somewhat by archaeologists doing community consultations, or more of them, before



1.5/ George Qulaut (speaking), Deborah Webster, and Gary Baikie address the Elders Conference participants.

they actually start their fieldwork. Community consultations can really help archaeologists not only show that they can be trusted but also provide information. With community involvement comes involvement of the elders. The elders in our communities are a wealth of information that is being under-utilized by most people.

It is very important to us that archaeologists consult with people and our elders while planning their fieldwork. Consultation is a two-way street. We feel that a lot can be learned from archaeologists in the way in which they involve Inuit in archaeology.

With the creation of Nunavut, archaeologists will deal with the Inuit Heritage Trust. Article 33.4.3 reads: "The Inuit Heritage Trust shall assume increasing responsibilities for supporting, encouraging, and facilitating the conservation, maintenance, restoration and display of archaeological sites and specimens in the Nunavut Settlement Area." We look forward to the coming changes of archaeology on Inuit lands.
Matna.

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Notes

1. Published obituaries include: Morrison 1996 for William Taylor; Gordon 1994 for Father Guy Mary-Rousselière; Lovis 1998 for Moreau Maxwell; and Laursen 1996 for Eigil G. Knuth.

2. These trends have been documented for Canada at large in *Bringing Back the Past: Historical Perspectives on Canadian Archaeology* (P. J. Smith and D. Mitchell 1998).



William Duncan Strong's "Band of Nugumiut Eskimos on Sylvia Grinnell River near Koojesse Inlet, Baffin Island", taken during the Rawson-MacMillan Subarctic Expedition of 1927

part 1

HISTORICAL PERSPECTIVES

The History of Arctic Ethnography:

Some Personal Reflections

ERNEST S. BURCH, JR.

Forty-odd years of experience with, or thinking about, the Arctic and its peoples have led me to develop a number of opinions about arctic ethnography. Since archaeology is intrinsically linked to ethnography in the Arctic, more so perhaps than anywhere else, the editors solicited this chapter for the historical perspective it brings to anthropological research in the Arctic.¹ In this review, I occasionally unburden myself of an opinion that some readers might find overly dogmatic. While I do not expect anyone to agree with me, I want you to understand the basis of my opinions. To that end, I begin by introducing you to some of the people I met and some of the experiences I had in the early years, which profoundly affected both my professional work and my outlook on life.

Before proceeding, I wish to specify what I mean by arctic and by ethnography. For purposes of this chapter, "arctic" means the Eskimo-Aleut part of the world, even though much of that is technically subarctic, and even though I am omitting much of the world that is technically "the Arctic." I use the term ethnography in its conventional sense, as the process of collecting information on sociocultural phenomena through systematic field research directed toward that end. Some people who are known primarily as archaeologists or linguists have conducted important ethnographic research as well.

Background

My interest in arctic ethnography began sometime in February or March of 1954 when an arctic explorer of



ancient vintage, Donald B. MacMillan (1918a, 1918b, 1927, 1943),² came to my school and presented an illustrated lecture on his arctic travels. This seventy-nine-year-old man was full of energy and enthusiasm, and he gave a real stemwinder of a speech. He also showed movies of country that was breathtakingly beautiful. At the end he said he was going to lead a scientific expedition to the Arctic that summer, and that he was looking for a couple of high school kids to take along.

The next day I wrote him a letter that said, in essence, "Here I am. Take me." And he did! Traveling in the two-masted schooner, the *Bowdoin*, with a crew of eleven, we went up the coast of Labrador, visiting Hopedale and Nain, and crossed Davis Strait to Greenland. Then we traversed the west coast of Greenland, visiting Kangaamiut and Sisimiut (then Holsteinsborg), and sailed on to Qaanaaq and Etah (then abandoned). We were stopped by ice in the southern part of Kane Basin and forced to turn around. We returned by way of Baffin Island (fig. 2.1) and Labrador. We experienced strong headwinds much of the time on the way north, and we got way behind schedule. On the day I was

supposed to report for football practice, we were just arriving in Pond Inlet on our way home. It was fantastic!

As a scientific expedition the trip was a farce, but it had a profound effect on me. Not only did I get to see some magnificent country, I met some fascinating people among the West Greenlandic (fig. 2.2), Polar, North Baffin Is-

land, and Labrador Inuit. I wanted to learn more about them. When we left in June, I wanted to be a field biologist, or what was then known as a “naturalist.” When we returned three months later, I wanted to be an anthropologist and work in the Arctic.

The next issue was, where could I go to learn how to be an anthropologist? My father advised me that for my undergraduate career I should attend a college known for the quality of its general program. Anthropology could take care of itself for a while, and I would need to go to graduate school for that anyway.

That view was very hard to argue with, so the question became, where should I go to college? There were a lot of good ones out there. For some reason I really wanted to get my teeth into something, which meant I wanted to go somewhere where I could write a thesis. A lot of colleges had a thesis as an option, but at most of them it was a privilege that had to be earned through exceptionally high grades. I was not sure that I could meet the requirements, so I went to Princeton, where one *had* to write a thesis even if one was carrying a D- average. And, of course, I intended to major in anthropology.

The main flaw in that plan was that Princeton did not have an anthropology major in 1956; indeed, it did not even have an anthropology department. Instead, there was a Department of Economics and



2.1/ Off Baffin Island, 1954

Sociology, with the two sections largely independent of one another in anticipation of a split. Anyone interested in anthropology was advised to major in sociology. In my class of some 1,200 people, there were only seven sociology majors. To my knowledge, during the four years I was there, I was the only student in the entire university, undergraduate or graduate, who had any interest whatsoever in anthropology.

The only anthropology course Princeton offered at the time was introductory cultural anthropology, so I took it. Despite the fact that it was taught by a rising young star who later became president of the American Anthropological Association, and who was also a fine person, I found it very boring. Field biology started to look kind of interesting again.

However, I had to take several sociology courses in order to satisfy the requirements of my major. That is where I got lucky. It turned out that many of the sociologists at Princeton in the late 1950s were oriented to both cross-cultural and historical research. None of them knew anything about arctic peoples, but my idea of writing a thesis on the eighteenth-century Labrador Inuit was fine with them.

The anthropology I had encountered in my introductory course featured the study of social forms and conjectural history. The sociologists, however, were interested in social process. They were structural-

functionalists who wanted to know how social systems operate, and why they operate the way they do—*without* committing the fallacy of eufunctional teleology. They were also interested in the study of social change. I do not think I attended a single sociology lecture or participated in a single sociology seminar in four years at Princeton where structural-functional analysis and the analysis of social change were not presented as a single, cohesive package. Many of my professors had studied under or were influenced by Talcott Parsons, who was alleged to be an antievolutionist, but Parsons visited Princeton while I was there and gave a talk whose central thesis was that the study of evolution was the wave of the future. Practically every member of the department was an evolutionist. The interesting questions were how and why social evolution occurred, not whether it had happened, and not whether it was a subject worthy of study. Their approach excited me then, and it still does.

In 1959, between my junior and senior years, I returned to Labrador for two months to get information for my thesis. I can hardly believe now

that I did it, because I had very little money, and I did not have the faintest clue about what I was doing. I never asked permission to come, nor did I inquire about how I might be received. I just went. But, once again, I was lucky. I was able to spend some time in the Inuit communities of Hebron, Nain (fig. 2.3), Hopedale, and Makkovik.³ I learned a great deal and had a wonderful time.

Also on the coast that summer was H. Anthony “Tony” Williamson. Tony had a B.A. from Dartmouth and was working on an M.A. in geography at McGill University. Although five years had passed since I had first acquired my own interest in the Arctic and its

people, Tony was the first person I ever met, except for Admiral MacMillan, who shared those interests. Among other things, Tony brought my attention to the existence of the Stefansson Collection at Dartmouth, and told me about a friend of his named Don Charles Foote. At the time, Foote was doing research in Point Hope, Alaska.

I returned to college in September, wrote my thesis, and graduated on schedule. One of my problems that year, in addition to writing the thesis, was deciding whether I should go to graduate school in anthropology or in sociology. For some reason, I happened to be attracted by the notion of general anthropology, in the North American sense. Interestingly, the sociologists at Princeton strongly encouraged me

to do my anthropology at the University of Chicago. That was fine with me. Chicago had not only outstanding cultural anthropologists, but also required basic training in linguistics, archaeology, comparative anatomy, and genetics. So, I went to Chicago. But I am getting a bit ahead of myself.



2.2/ *Greenlandic girl, 1954*

While I was working on my thesis on Labrador, I paid a visit to the Stefansson Collection. There, I got to meet not only Vilhjalmur Stefansson himself but also Alan Cooke, who was the assistant librarian. Cooke asked me if I would like to spend a year in Alaska working for Don Foote. It was a dumb question. Cooke wrote to Foote on my behalf. A few weeks later, I received a letter from Foote, who was still in Point Hope. He described his research program there, which was part of what we would now call an environmental impact assessment and which was being carried out for the Atomic Energy Commission's Project Chariot.⁴ He was looking for a research assistant.



2.3/ *Miriam Torrarak, Nain, 1959*

A rather amusing exchange of letters followed. Foote essentially accused me of being a wimp, and I suggested that he was a liar and a jerk. However, we worked it out, and he decided I wasn't as bad as he first thought. Unfortunately, Foote was among the leading critics of the planned nuclear program of Project Chariot, so the members of the Atomic Energy Commission were not feeling very kindly toward him at the time. Accordingly, he did not get a contract for the position for a research assistant. Instead, Doris Saario, who had been in charge of the Project Chariot study at Kivalina in 1959–60, was retained, but she was looking for an assistant. She hired me on Foote's recommendation. As a result, I spent the period from October 1960 to late August 1961 in Kivalina learning about native life in general and subsistence in particular. During most of that period, I worked on the Project Chariot study (Saario and Kessel 1966), but for the last three months I was on my own. Then I headed for graduate school at the University of Chicago.

Shortly after my arrival in Chicago, I started to find the cultural anthropology courses almost as boring as my introductory course had been at Princeton. They were focused on form, not process, and social evolution was an alien concept. Even worse, I was told that structural-functional analysis and the study of social

change are mutually exclusive endeavors. Evidently, my sociology professors at Princeton had been too ignorant to know that, which is why they combined the two so successfully.

Later, I learned that many structural-functional analyses were indeed characterized by the flaws the critics claimed they had. I also learned that many researchers did proffer teleological explanations of social phenomena. But the mistakes the critics seized on resulted from erroneous application, not

from flaws in the basic approach. When they threw out the admittedly dirty bathwater, they threw out a very healthy baby along with it.

Anyway, Lewis Binford was pontificating at Chicago when I was there, so the archaeology was quite interesting.⁵ Rather than taking courses in my own specialty, I ended up taking a lot of courses in anthropological archaeology, a subject not included in the curriculum at Princeton when I was there. Those courses and a summer (1962) spent working for Elmer Harp (1964a, 1976b; Harp and Hughes 1968) on a Dorset Culture site at Port au Choix, Newfoundland (fig. 2.4), were not enough to qualify me as an arctic archaeologist, but they did enable me to claim permanent status as a gadfly (e.g., Burch 1988b) in that field.

For the past twenty-one years, I have made my home in south-central Pennsylvania. That seems to me to be a perfectly good vantage point from which to ponder the history of arctic ethnography, and to that I now turn. The perspective I bring to the comments I make below derives partly from this location, where I am remote not only from the Arctic, but also from academia. My perspective is also influenced by the experiences I have just summarized and by my fundamental interest in social process rather than social form.⁶

Historical Overview

I turn now to the primary subject of my chapter. For purposes of this review, I divide the history of arctic ethnography into three broad periods: early, meaning before 1900; intermediate, from 1900 to 1954; and recent, from 1954 to the present.

Early Period

Before 1900, there were very few arctic ethnographers as I have defined those terms. Indeed, there are only eighteen people I would include in this category. There were lots of explorers and missionaries who recorded information on Eskimo social life and customs, but very few of them engaged in systematic research directed toward that end. Even the research that was done was carried out, in most cases, as an adjunct to some other duty.

For example, several of the early ethnographers were missionaries: Hans Poulsen Egede (1745), David Crantz (1767), and Otto Fabricius (Holtved 1962) in Greenland; Émile Petitot (1876) in the Mackenzie Delta; and Ivan Veniaminov (1984) in southern Alaska. Although technically he does not belong in it, I will add Harrison Thornton (1931) to the list of early missionary ethnographers because his book on Wales, Alaska, covers a wide variety of subjects and because it was informed to a significant extent through participant-observation. Most of the early missionary-ethnographers acquired their expertise through long experience in the country. Petitot and Thornton were simply keen observers who recorded what they saw during relatively short periods of time.

Several other early ethnographers were either natural scientists of one variety or another, or people who were working on natural science projects. In this

category I include John Murdoch (e.g., Murdoch 1892), who worked at Barrow during the International Polar Expedition of 1881–83; Gustav Holm (1914), who led the Danish Cartographical Expedition to East Greenland in 1883–85; Edward Nelson (1899) and Lucien Turner (1894), who were biologists; F. F. Payne (1889) and Robert F. Stupart (1887), who wrote about the Inuit in northern Quebec while they were members of the Canadian Hudson Bay Expeditions of 1884–85; Heinrich Holmberg (1985), who was a mining specialist; Hinrich Rink (e.g., Rink 1875, 1877), who began his long career in Greenland as a glaciologist; and John Simpson (1875), who was a surgeon on one of the ships in the Franklin search expeditions in northern Alaska. All of these people were trained scientific observers. Nelson and Simpson spent several years in the Arctic; Rink ended up spending decades in Greenland. Three of them—John Murdoch, Edward Nelson, and Lucien Turner—were specifically directed to bring back items of material culture and information relating to native life.

There were two other early ethnographers whom I would have to call “off-the-wall” types. One was John Kelly (Wells and Kelly 1890), a kind of adventurer and whaler who arrived in northwestern Alaska in 1884. By the time Lieutenant Roger Wells got him to summarize



2.4/ Elmer Harp and crew in Cow Head, Newfoundland, 1962

on paper what he knew about the peoples of northern Alaska and Chukotka, Kelly had lived in the region for six years, had traveled extensively, and had acquired considerable knowledge of native life. The other was Henry D. Woolfe (1893), a journalist who had arrived in northwestern Alaska even before Kelly did. Woolfe, who also traveled widely, wrote the descriptive piece on the people of the region for the 1890 census (some of which was plagiarized from Kelly).

That leaves only one person whom I have not mentioned, and that, of course, is Franz Boas (Boas 1888; Cole and Müller-Wille 1984). Boas was a cultural geographer who may have been the first person ever to go to the Arctic with a social science objective as his top priority. He set out specifically to collect information on human-environment relationships and on the factors affecting human migration. In addition to other things that is exactly what he did.

In many respects, the research of these pioneer ethnographers was extraordinary. They were working in difficult physical and social circumstances that were wholly outside their experience or training, and they were working in the field of ethnography, which was itself in a primitive state of development. Because of their holistic approach, they provided information on a remarkably wide variety of topics, and they covered a huge geographic area. When the early period came to an end, the central Canadian Arctic was the only area that remained to be surveyed ethnographically.

I recall being very frustrated during my student days by the body of literature these ethnographers produced. When I reviewed that literature recently, I could see why. Here they were, as close to the cutting edge of European contact as any ethnographers would ever be, yet they hardly had anything to say about the social dynamics of traditional Eskimo societies. A great deal can be determined about Eskimo life based on the information these ethnographers acquired, but there is no way to know, relying solely on the evidence contained within the documents they produced, how an Eskimo village or society really worked.

The good news is that they left a lot for the rest of us to do. For example, Franz Boas's work on Baffin Island in 1883-84 was outstanding. However, it was not until more than a century later, when Marc Stevenson (Stevenson 1993) wrote his thesis on Cumberland Sound Inuit social structure, that it became possible to say, at a fairly sophisticated level, how the system probably worked when Boas was there, more than a century earlier.

Intermediate Period

The intermediate period was ushered in by Franz Boas (1901, 1907) with the publication of his volume on the Eskimos of Baffin Land and Hudson Bay. This two-part document was based on notes collected at his request by the whaling captains George Comer and James Mutch and by the missionary E. J. Peck. This work actually began in 1898, but continued to 1902. It contained a large amount of rather miscellaneous information, but it served as a useful complement to his own earlier fieldwork. After completing it, Boas turned his powerful attention to the Northwest Coast and never returned to arctic research.

The first part of the intermediate period was marked by a series of major expeditions. They began with the first phase of the Jesup North Pacific Expedition (Fitzhugh 1994a; Freed et al. 1988), and ended with the Seventh Thule (Rasmussen 1933a, 1933b) and the Danish-American Alaska (Birket-Smith 1953:1) expeditions in 1933. In-between were the Danish Literary Expedition of 1903-04 (Mylius-Erichsen and Moltke 1906); the Kamchatka-Aleutian Expedition of 1909-10 (Jochelson 1912); the Fourth, Fifth (Rasmussen 1926, 1927; Rasmussen et al. 1925), and Sixth (Rasmussen 1932b) Thule expeditions;⁷ the Crocker-Land Expedition of 1913-17 (MacMillan 1918b); the Carnegie Magnetic Expedition around the Labrador Peninsula in 1914 (Hawkes 1916); the Stefansson-Anderson Expedition of 1908-12 (Stefansson 1914b); and the Canadian Arctic Expedition of 1913-18 (Anderson 1915-17; D. Jenness 1916; S. Jenness 1991; Stefansson 1921a). The

ethnographers involved included many who became premier practitioners of arctic ethnography: Kaj Birket-Smith (e.g., Birket-Smith 1924, 1928, 1929, 1953); Diamond Jenness (see below); Knud Rasmussen (1908, 1929, 1930a, 1930b, 1931, 1932a; Ostermann 1942; Ostermann and Holtved 1952)⁸; Vilhjalmur Stefansson (1913a, 1914b); and William Thalbitzer (1914, 1923, 1941). Others were Waldemar [Vladimir] Bogoras (1904–1909, 1913); Edward Curtis (1930); Walter Ekblaw (1921, 1927, 1928, 1947, 1948)⁹; Ernest Hawkes (1914, 1916); Waldemar [Vladimir] Jochelson (1933); and Hans Peter Steensby (1910). Therkel Mathiassen, known primarily for his archaeological work, also did important ethnographic work during the Fifth Thule Expedition (Mathiassen 1928).

After the Great Depression, “expeditions” tended to become one- or two-person affairs. They were carried out by such people as Jean Gabus (1940, 1940–41, 1941); Robert Gessain (1935, 1937a, 1937b); J. L. Giddings (1941, 1956, 1961); Irma and John Honigmann (Honigmann and Honigmann 1953; J. Honigmann 1951, 1952); Margaret Lantis (see below); Alexander and Dorothea Leighton (Leighton and Leighton 1983); Froelich Rainey (see below); N. B. Shnakenburg,¹⁰ and Paul Émile Victor (1938, 1939, 1940).

This work filled in the major geographic gap of the central Canadian Arctic and started to fill in some of the holes remaining in our knowledge of southwestern and southern Alaska. In addition, it added a wealth of detail to what was already known, and it produced for the first time in the Arctic examples of what I consider to be world-class ethnography. In particular, the work of Diamond Jenness and Margaret Lantis can be mentioned as being in that category.

The ethnography of the intermediate period is well known. Thus, instead of reviewing it further, I am going to offer a few brief opinions on the work of three of the individuals who were involved in it.

The first ethnographer I wish to discuss is Vilhjalmur Stefansson¹¹ As Stefansson was fond of noting himself (e.g., Stefansson 1913a:175), when he arrived among

the Copper Inuit he already spoke Inuktitut with some fluency. He was thus in the unique position of being able to speak the language of the people he was going to study before they had ever even seen a Westerner. From that perspective, his ethnographic work among the Copper Inuit (e.g., Stefansson 1913a, 1914b) must be regarded as a disappointment. I would say much the same about his work in the Mackenzie Delta and northern Alaska (e.g., Stefansson 1908, 1909, 1910, 1912–13, 1914a; also 1913c, 1914b). His results, while informative and important, were far below what one might expect, given his training and the extraordinary opportunities he had in both regions. Stefansson was too interested in being an explorer and an iconoclast (e.g., Stefansson 1921b, 1956), and not interested enough in being an ethnographer, to put together a systematic ethnographic account of an Eskimo population. One subject he did treat with insight and attention, however, was Inuit religion. We may still read with profit what he had to say on that topic (Stefansson 1913b, 1913c, 1953).

The second person I want to say something about is Diamond Jenness. Jenness’s work represents the very highest standards of ethnographic endeavor. He wrote several monographs (Jenness 1922, 1923b, 1924a, 1924b, 1928b, 1946; Roberts and Jenness 1925), several articles (e.g., Jenness 1917, 1921, 1923a), and a popular book (Jenness 1928c) on his research among the Copper Inuit. His work alone qualified that group for inclusion in the Human Relations Area Files from the day they were founded. His *Life of the Copper Eskimo* is a masterpiece, written in an engaging style, but crammed full of information on how the Copper Inuit system worked. In terms of theoretical sophistication, it was the equal of anything that was being produced anywhere in the world at that time.¹²

Sometimes I muse over the fact that Jenness did his Arctic fieldwork at exactly the same time Bronislaw Malinowski (1967) was doing his first research in the Trobriand Islands. If Jenness had published books with nifty titles, like Malinowski did,¹³ and if he had been a

professor at a major university (e.g., the London School of Economics) like Malinowski, arctic ethnography would have had a much greater impact on world anthropology than it has had. But he stuck to museum work at the National Museum of Canada and to museum monographs, and few who were not arctic specialists knew or cared much about his work.

The final person from the intermediate period whom I want to comment on is Froelich Rainey. Rainey was primarily an archaeologist, but he spent nine months in 1940 doing ethnographic research in Point Hope, Alaska. The result was the publication of three articles (Rainey 1940, 1941a, 1941b) and a very short monograph (Rainey 1947). Unfortunately for us, World War II directed Rainey's attention elsewhere, and he never returned to arctic research. However, I have had the privilege of examining all of his field notes. I can tell you that during the few months Rainey spent in Point Hope, he collected enough material for a major monograph, one the size and scope of Robert Spencer's (1959) volume on Barrow, but perhaps even better informed. His published work was just an outline of the information he actually had. Some of his notes are accessible in the Alaska and Polar Regions Department at the University of Alaska Fairbanks. I recommend that you take a look at them. If you do, then the potential inherent in his work may ultimately be realized.¹⁴ This brings me to 1954.

Transitional Year

Clearly, 1954 was a threshold year in the history of arctic ethnography, since that is the year I first went to the Arctic. Personal views and jokes aside, the early 1950s really were a time of major transition in the field, because it took until then to get the effects of World War II out of the system. With the help of Igor Krupnik and James VanStone, I compiled a list of all the people who were actively involved in arctic ethnography during that year. The list contains 29 names. We probably missed a couple of Danes, but otherwise I think the list is pretty complete.

I will not take the space to present the list, but it is appropriate to say a bit about it because the names alone say much about the situation existing at the time. Important names that were *missing* include Kaj Birket-Smith, Jean Gabus, Diamond Jenness, Therkel Mathiassen, Froelich Rainey, Vilhjalmur Stefansson, and William Thalbitzer. They were all still alive, but were either no longer interested in the Arctic or else no longer doing or writing about ethnographic research.

Two other important names missing from the list are those of James Vanstone and Wendell Oswalt. Both began their ethnographic careers, as opposed to their archaeological careers, in 1955.

Important names that *are* on the list include Robert Gessain (1967, 1969, 1979–80; Gessain and Robert-Lamblin 1974, 1975); Erik Holtved (1951, 1958, 1967); John and Irma Honigmann (Honigmann and Honigmann 1965; J. Honigmann 1962, 1965a, 1965b); Margaret Lantis (see below); Jean Malaurie (1956, 1974; Malaurie et al. 1952); Dorothy Jean Ray (1960, 1963, 1964, 1967, 1971) and Robert Spencer (1953, 1955, 1956, 1958, 1959, 1960, 1967–68, 1968, 1972, 1984; Spencer and Carter 1954). Just starting his fieldwork in Gambell was Charles Campbell Hughes (1957, 1958a, 1958b, 1960, 1966, 1968, 1974). Also included are people most of you probably never heard of, such as Claude Desgoffe (1955a, 1955b); Marjorie C. Findlay (1953, 1955) and Anna Smoliak. In any event, it was the beginning of an exciting time in arctic ethnography, and to that I now turn.

Recent Period

During the early and intermediate periods of arctic ethnography, the number of researchers who were active at any given time rarely exceeded half a dozen. The number who were active even during periods covering half a century rarely exceeded two dozen. During the recent period, the number of researchers rose by at least an order of magnitude. At Laval University in Quebec, there are probably more active arctic ethnographers right now (i.e., in 1995)

than there were in the entire world in 1950. World-wide, we must now speak in terms of hundreds of arctic ethnographers, not dozens.

The rapid expansion in the number of arctic ethnographers began in the mid-1950s. In Greenland, early interest was in the social changes attending the island's opening to the outside world. Research focused on ethnic identity (e.g., H. Kleivan 1969–1970; I. Kleivan 1969–1970; Petersen 1992), economic development (e.g., Christiansen 1966; Ørvik 1976), and the development of political institutions (e.g., Benoit and Martens 1992; Goldschmidt 1963; Nooter 1976). Subsequently, attention turned to such topics as the consequences of home rule (e.g., Foighel 1980; Motzfeldt 1987) and land-use planning (Greiffenberg 1992), although otherwise the themes remained about the same (e.g., Brøsted and Gulløv 1977; Høyem 1988; Lynge 1988; Schechter 1983). Much of the work had a definite applied orientation, although some more classic work—mythology (e.g., Kleivan 1960; Savard 1966), kinship (e.g., Søby 1977–1978), and village organization (Nuttall 1992), for example—was also carried out. Most of the work in Greenland was done by Danish scholars, although the French (e.g., Robbe 1994; Victor and Robert-Lamblin 1989, 1993) maintained a strong presence in eastern Greenland,¹⁵ and others have been involved from time to time.¹⁶

In Canada, the expansion of arctic ethnography in the 1950s arose out of the sad state of many Canadian Inuit groups, especially the Caribou Inuit, as reported by Farley Mowat (1954, 1959) and photographed by Richard Harrington (1954). The Canadian Department of Northern Affairs and Natural Resources (later the Department of Indian Affairs and Northern Development) sponsored a number of studies, including several area economic surveys (e.g., Abrahamson 1964; Anders 1965; Bissett 1967; Meldrum 1975; Usher 1966), as well as investigations of community structure (e.g. Dailey and Dailey 1961; Ferguson 1963; Mailhot 1968; VanStone and Oswalt 1959; Willmott 1961) and social problems (e.g., Brody 1970; Clairmont

1962; Lubart 1970). Most of the resulting publications were produced in limited quantities and were distributed to a restricted set of readers; they constitute the foundation of Canada's extensive "gray literature" on the Inuit.

As in Greenland, much of the post-1954 work in Canada has had an applied orientation. However, there also have been more classic studies, such as Balikci's (1970) volume on the Netsilik, Damas's (1963) study of the Igluligmiut, Guemple's (1966) work in the Belcher Islands, Graburn's (1964) and Saladin d'Anglure's (1967) separate researches in northern Quebec; David Stevenson's (1972), Marc Stevenson's (1993), and George Wenzel's (1981) studies in Baffin Island; Ben-Dor's (1966) research in Labrador; and F. G. Vallee's (1967) work in central Keewatin. One important research focus was the Inuit land claims (Brice-Bennett 1977; Freeman 1976; Riewe 1992) and the foundation on which they were based. Social change is an enduring topic (e.g., Condon 1987; Matthiasson 1992; McElroy 1973; Wenzel 1991), as is its counterpart—the *maintenance* of traditional values and lifestyles in a rapidly changing world (e.g., Ames et al. 1989; Freeman 1992; Freeman and Carbyn 1988). Beyond that, recent ethnographic research in Canada has been too voluminous and too specialized to attempt to summarize here.

In Alaska, ethnography really started moving in the 1950s with Spencer's (1959) monograph on the North Slope, and the three famous community studies—VanStone's (1962) on Point Hope, Oswalt's (1963) on Napaskiak, and Hughes's (1960) on Gambell. Research expanded dramatically during the 1960s, particularly in northwestern Alaska, which was probably the most exciting place to be in all of the Arctic at that time. We are all familiar with the old joke that, in the 1930s, the average Navajo family consisted of two parents, four children, and an anthropologist. Things did not go quite that far in northern Alaska, but just about every Inupiaq village had one or more resident anthropologists. By my count, during the decade of the 1960s, at least

thirty ethnographers did some kind of research in twenty-six Inupiaq villages.¹⁷ Several others were working farther south, but the density of researchers was nowhere near as high in southwestern and southcentral Alaska as it was in the north.

One of the interesting things about the work in Alaska during the 1960s was that, after Project Chariot was over in 1961, most researchers worked independently of both institutional projects and of one another. It was only later, with the Alaska Native Claims Settlement Act of 1971, the state subsistence law, oil exploration, and the Alaska National Interest Lands Conservation Act, that government-sponsored research became important. Since 1970, an enormous amount of ethnographic research has been commissioned or carried out by the Alaska Department of Fish and Game, Division of Subsistence (see 1994 for references and abstracts; also Fall 1990); the U.S. Bureau of Indian Affairs¹⁸; the U.S. Bureau of Land Management, Alaska Outer Continental Shelf Office (e.g., Davis 1979; Ellanna 1980; Fienup-Riordan 1982; Policy Analysts Ltd. 1980; Worl Associates 1978); the U.S. National Park Service (e.g., Anderson et al. 1977)¹⁹; and the U.S. Minerals Management Service (e.g., Braund 1988a; Luton 1985; U.S. Minerals Management Service 1988, 1992; Worl and Smythe 1986). The volume of the "gray literature" resulting from all this work even exceeds that produced earlier in Canada.

Government-sponsored research is by no means the only type of ethnographic work done in Alaska since 1970, however. Individuals pursuing their own research interests have produced dozens of studies. These range from comprehensive descriptions of native life (e.g., Gubser 1965) to studies of social change (e.g., Chance 1990; Milan 1964; VanStone 1967), kinship (e.g., Burch 1975; Fienup-Riordan 1983), folklore (e.g., Hall 1975), gender (e.g., Chance 1988; Jolles 1991), ritual (e.g., Fienup-Riordan 1994), inter-ethnic relations (McNabb 1985), and the construction of skin boats (Braund 1988b), to list just a few of the topics that have been investigated.

Ethnographic research in the Soviet Union took longer to get started after World War II than it did in the rest of the Arctic. It was not until the 1970s that important research was underway, especially in the areas of social organization and ecology. Important names to mention here are Chlenov (e.g., Chlenov 1973, 1983) and Krupnik (e.g., Krupnik 1981, 1993), but several others, including a number of linguists, have been involved as well.

Most of the ethnography that has been done during the recent period has been more specialized than that carried out earlier. Instead of attempting to describe all aspects of a society and its setting within a single monograph, researchers have focused on the kinship system, ecology, suicide, etc. This makes sense because most researchers have been working in ground that had been plowed, even if not disked and harrowed, before they arrived.

As attendance at the annual meetings of the Alaska Anthropological Association and especially at the biennial Inuit Studies Conferences attests, arctic ethnography in 1995 is a dynamic field, more so than at any previous time. Another, less positive indicator that arctic ethnography is coming of age is the growing tide of petty jealousy, personal vendettas, and peer review sabotage that has developed over the past few years. If this trend continues, it will not be long before we achieve the stature acquired long ago by the Oceanists and Africanists.

A third indicator of the progress we have made is the volume of material one must examine when writing even a relatively simple article. Now it takes me nine- or ten-times as long to research a twenty-five-page paper as it did when I was a graduate student. For a while I thought it was because I was getting senile. Then I realized that, over the past thirty years or so, the quantity of available information has increased by an order of magnitude.

The work that is going on today ranges from pathetic to outstanding. I am pleased to note, however, that the mode is definitely skewed in a positive

direction. In my terms, this means that the emphasis is increasingly on process rather than form, and that several people are doing structural-functional analysis even though they do not know it. There may be hope after all.

Before leaving my discussion of the recent period, I wish to focus attention briefly on the work of two individuals. The first is Margaret Lantis. It is presumptuous of me to attempt to assess Lantis's work in a few sentences. It spans more than a half a century and covers an awesome variety of subjects. Not to single it out for attention, however, is unthinkable. Lantis's (1946) monograph on the social culture of the Nunivak Eskimo was holistic, in the old tradition of arctic ethnography, but the level of analysis was more sophisticated than that of most of her predecessors. To her monograph she subsequently added more specialized studies of various aspects of Nunivak life (e.g., Lantis 1953, 1960), analyses of the Aleut social system (Lantis 1970) and Kodiak Island mythology (Lantis 1938b), comparative studies on a wide variety of subjects and regions (e.g., Lantis 1938a, 1947, 1950, 1959a, 1959b, 1990), and studies of health issues (Lantis 1967, 1981) and social change (e.g., Lantis 1952, 1966, 1972, 1973), among other topics. Altogether, Lantis arguably has written on a greater variety of research topics than any other Arctic ethnographer.

The second person I want to single out for attention is David Damas. Damas was trained by Fred Eggan at the University of Chicago and was profoundly influenced by him. Initially, this gave his work something of a formalist quality and a somewhat dated appearance. But these impressions are misleading. Damas, like Eggan, was too interested in how systems operate to limit himself to formal analysis. His work on Iglulingmiut kinship and local groupings (Damas 1963, 1964) was the most sophisticated analysis of Eskimo kinship ever written up to that time. Subsequently, Damas expanded his geographic range to include the Netsilik and the Copper Inuit (Damas 1972b), in addition to the Iglulingmiut, and many of his publications

(e.g., Damas 1966, 1968, 1969a, 1969b, 1971, 1972a, 1972b, 1975a, 1975b, 1975c, 1988) involved comparisons of the three. His interests broadened to include ecology, social change, and a variety of other subjects. Damas eschewed theoretical fads and fancy titles, but he kept his eye on fundamental issues that will continue to be important far into the future.

Conclusions

Where does all of this leave us? What impact has arctic ethnography had on sociocultural anthropology in general? The answer is, precious little. Among those few anthropologists who work in other lands who are aware we even exist, we are a laughingstock. The only Arctic ethnographer whose work is regularly cited outside our own narrow circles is Jean Briggs (e.g., Briggs 1970, 1979, 1982, 1991). Briggs is justifiably recognized for her brilliant work on family relationships, socialization, and emotional expression. While others have published important findings as well, their work has been ignored.

When pondering our minimal impact on others, it is useful to pause and consider areas where contributions might be expected of us. Obviously, we can contribute little or nothing to debates about descent systems, to the understanding of peasant revolts, or to the analysis of agrarian economies. And those are important subjects. There are then, obviously, some inherent limitations on the kinds of subjects we can study, but that problem exists everywhere.

A much more serious charge is that Arctic ethnographers have been "bypassed by contemporaneous theoretical developments" (Riches 1990:73). Given the faddism involved in most of the so-called theoretical developments of the past thirty years, however, I am not so sure that is a bad thing. Every few years or so someone has come up with a clever idea, and everyone else has jumped on the bandwagon. Then, after a brief flurry of research and publication on that topic, someone has found a flaw in the idea, the political climate has changed, or everyone has gotten bored



2.5/ Lawrence Sage of Kivalina after a productive day of seal hunting in June, 1964

with it, and that fad has been abandoned in favor of another one. For some reason, most Arctic ethnographers have not been trendy; they have pretty much stuck to fundamentals. If that is what is involved in being bypassed by contemporary theoretical developments, then I say, good for us.

But there are few truly general areas of theoretical significance where we cannot say something of importance. Actually, there are few areas where we have *not* said something of importance. But either we have failed to address the general issues explicitly in our publications, or we have published in regional rather than in national or international journals. Most of the time we have done both. As a result, few outside of arctic ethnography even see, never mind read, what we have written. It is pretty difficult to make an impact under those conditions.

There are two areas in particular where Arctic ethnographers have an enormous amount to contribute to world anthropology: social change and the structure of small-scale societies. I will comment briefly on each.

The subject of social change is all-encompassing, since every aspect of a social system can and does change over time. But few peoples in the world have experienced such profound changes, even in world history, as the peoples we study. Many of them are hurting as a result. Much of the work done in the Arctic over the past twenty-five years has, in fact, dealt with change. But most of it has been oriented to case studies rather than to the search for general principles. Until we devote more serious attention to general principles, our work will have little impact on research done elsewhere.

Most studies of social change, of course, have implications for applied anthropology. Indeed, there has been considerable progress in this area in the Arctic. I was pleased a few years ago to see the work of several of our Alaskan colleagues featured in an issue of *Practicing Anthropology* (Feldman and Langdon 1982); and the work of others (e.g., Kruse 1991; McNabb 1993) has appeared in *Human Organization*. That is a good start, but much more needs to be done. Make no mistake about it: the future of arctic ethnography

lies in this area, not in more traditional forms of ethnographic research.

The second area where arctic ethnography has an enormous amount to contribute to world anthropology is in the understanding of small-scale societies, particularly hunter-gatherer societies (fig. 2.5). Unfortunately, most of our contributions here have been made already. This is because it is almost impossible now to get information on Arctic peoples as they were before contact, or during the early stages of contact, through ethnographic research. The knowledge simply has been lost. I have collected enough field data over the last thirty-five years to keep me going for the rest of my professional career. Newcomers, however, are much more out of luck. Only in a few areas, such as southwestern Alaska, where the oral tradition is still very strong, can new information still be acquired by means of ethnographic research techniques. From now on, it will be largely up to ethnohistorians and (heaven help us!) archaeologists to provide us with new information on hunter-gatherer societies.

Before concluding, I want to note that the field of hunter-gatherer studies is rapidly becoming polarized. On the one hand are the students of simple hunter-gatherers, such as the Basarwa, the Australian Aborigines, and the Hadza. On the other are the students of complex hunter-gatherers, such as the Tlingit, the Kwakwaka'wakw, and the Calusa. Simple and complex hunter-gatherers are so different from one another that the individuals who study peoples at one extreme have almost nothing to say to those who work at the other. The international conferences on hunter-gatherer societies—the so-called CHAGS series (Burch 1994:446)—have been dominated by students of simple hunter-gatherers. As a result, students of complex hunter-gatherers have begun to have separate conferences of their own (e.g., Price and Brown 1985).

But, if one starts with the East Greenlandic and Polar Inuit and works progressively westward across the top of the continent, and then moves counter-clockwise around Alaska to the Aleutians and Kodiak

Island, one finds that the gap between the simplest and the most complex hunter-gatherer societies in the ethnographic record is a continuum, not a dichotomy. The people who study the societies at the extremes of hunter-gatherer variation are evidently too ignorant to realize this fact, which is why others have ignored our work. But it must also be said that we have been too reluctant to bring it to their attention. It is time to correct this deficiency.

Notes

1. This chapter was originally presented as a keynote address at the 22nd Annual Meeting of the Alaska Anthropological Association on March 24, 1995.

2. See also Allen (1962) and MacMillan (1948).

3. Here I was doubly fortunate because the mission, school, and store at Hebron were all permanently closed during the summer of 1959, which led to a mass southward migration of the native population.

4. The results of the Project Chariot studies were published in a substantial volume edited by Wilimovsky and Wolfe (1966). An excellent history of Project Chariot was recently written by O'Neill (1994).

5. At this point Binford had not yet begun his ethnographic work in Anaktuvuk Pass, Alaska (see, e.g., Binford 1976, 1978a, 1978b, 1980; Binford and Chasko 1976).

6. I have never read or heard of a clear distinction being made between social and cultural anthropology, which is probably why the compound term *sociocultural* appears so often. It seems to me that those interested primarily in social forms usefully could be called *cultural anthropologists*, while those interested in social process could be called *social anthropologists*.

7. The First, Second, and Third Thule expeditions did not involve ethnographic work. The fourth one did, but I have been unable to find a report devoted to it.

8. The work of Knud Rasmussen has been reviewed many times, most recently in Kleivan and Burch (1988).

9. It is appropriate to provide a bit of perspective here. Ekblaw did his research as a member of the Crocker-Land Expedition of 1913–17,

which was led by Donald B. MacMillan. My own first trip to the Arctic took place forty years later, also under MacMillan's leadership.

10. Shnakenburg's material was not published, apparently because it was suppressed by the Soviet regime. It did, however, serve as the basis of Menovshchikov's (1964) article on the Eskimo. According to Lydia Black (personal communication 1995), it is on file in the *Kunstkamera* at the Museum of Anthropology and Ethnography in St. Petersburg, Russia.

11. Stefansson's life and work were reviewed in a special issue of *Polar Notes* published in 1962. Also see Diubaldo (1978).

12. See Jenness 1918 and 1929a. Jenness (1957) also made significant contributions to the ethnography of northern Alaska.

13. For example, Malinowski's *Argonauts of the Western Pacific* (1922), *Sex and Repression in Savage Society* (1927), and *Coral Gardens and their Magic* (1935).

14. As far as I know, Lowenstein (1992, 1993)

and I (Burch 1981) have been the only ones to take advantage of this resource.

15. For French research prior to 1975, see Perrot and Robert-Lamblin (1975).

16. The papers in a special issue of *Arctic Anthropology* published in 1986 include a representative sample of topics of recent interest in ethnographic research in Greenland.

17. The Inupiat (pl.) are the Inuit-speaking peoples of northwestern Alaska (Woodbury 1984). In the 1960s, their territory extended from Unalakleet on the south to Barter Island on the northeast, and included Little Diomedé and King Islands in the Bering Strait.

18. Most of the Bureau of Indian Affairs work has not been published, but can be tracked down.

19. In the 1970s and 1980s, most of the anthropological research sponsored by the National Park Service was archaeological in nature and was carried out through the Anthropology and Historic Preservation Cooperative Park Studies Unit, University of Alaska Fairbanks.

Elmer Harp's Contribution to Bush Archaeology

J. V. WRIGHT

Despite the fact that about 6 million square kilometers of Canada are currently covered by bush vegetation and another 5 million by tundra, the latter region has attracted far more archaeological attention. By the term "bush," I mean the Boreal Forest and Lichen Woodland vegetation provinces; these areas are generally equated with "bush" archaeology. The Tundra vegetation province, on the other hand, is usually identified with "arctic" archaeology. The geographical distributions of these three vegetation provinces, of course, have fluctuated through time (McAndrews et al. 1987; Ritchie 1987).

It is well known to bush archaeologists that arctic archaeologists will recoil to the north at the very sight of a scraggy outlier of black spruce trees. Bush archaeologists have a somewhat similar aversion to an arctic landscape of limestone shingle beaches, boulder fields regurgitated by glaciers, and scoured outcrops, all of which are sometimes covered by a diminutive vegetation that includes arctic giants like ground willow and towering, 5- to 6-inch-tall woolly louseworts (*Pedicularis lanata*). Given these circumstances, it is not surprising that, even though they are neighbors, arctic archaeology and bush archaeology, for the most part, have been treated as distinct and unrelated entities. As with all generalizations, however, there are exceptions and, in this instance, Elmer Harp is most definitely an exception. In fact, it would be an exercise in futility to attempt to classify Elmer Harp as either an arctic or a bush archaeologist because, unlike most northern researchers, he has kept his feet firmly planted in both regions.



In the spring of 1960, as the neophyte Ontario Archaeologist with the National Museum of Canada, I drove to New Haven in the company of Diamond Jenness, Scotty MacNeish, Larry Oschinsky, and Bill Taylor to attend the 25th Annual Meeting of the Society for American Archaeology at Yale University. During the meetings I met many eminent archaeologists. Included in their numbers were Elmer Harp, Fred Johnson, and Doug Byers, who collectively left me with the impression that the senior archaeologists from New England were not only outstanding scholars but also considerate gentlemen who provided considerable encouragement to a young archaeologist just learning the trade. This first impression was only enhanced with the passage of time. Having already made field plans for the first of a number of National Museum of Canada archaeological surveys in the Canadian Shield, I was naturally most interested in a symposium at the meetings that was organized by Jack Campbell (1962). The symposium addressed the question of archaeological relationships between the Arctic and the more southerly regions. Paradoxically, this symposium was, and still is, one of the few concerted attempts to

relate arctic archaeological evidence to developments in the south. Among the presentations was a paper by Elmer Harp (1962) on the culture history of the Central Barren Grounds, which was to have major implications for bush archaeology in the future.

A distinctive feature of all of Elmer Harp's northern research is its concentration in regions characterized by past human occupations that involved a number of different archaeological cultures. Some might suggest that this selection of field research areas, which provided exceptional opportunities for studying such important matters as cultural replacement and interaction, was a matter of pure luck, but I believe that it was more likely the product of a perceptive scholar who founded his research strategy upon broadly based anthropological considerations. This approach included a strong sense of the necessity of viewing past cultures in relation to their environments and of trying to determine how they adapted to changing environments. His work represented the first concentrated archaeological reconnaissances in a number of regions. While he carried out relatively little excavation, his initial findings and interpretations pointed the way for subsequent northern archaeologists.

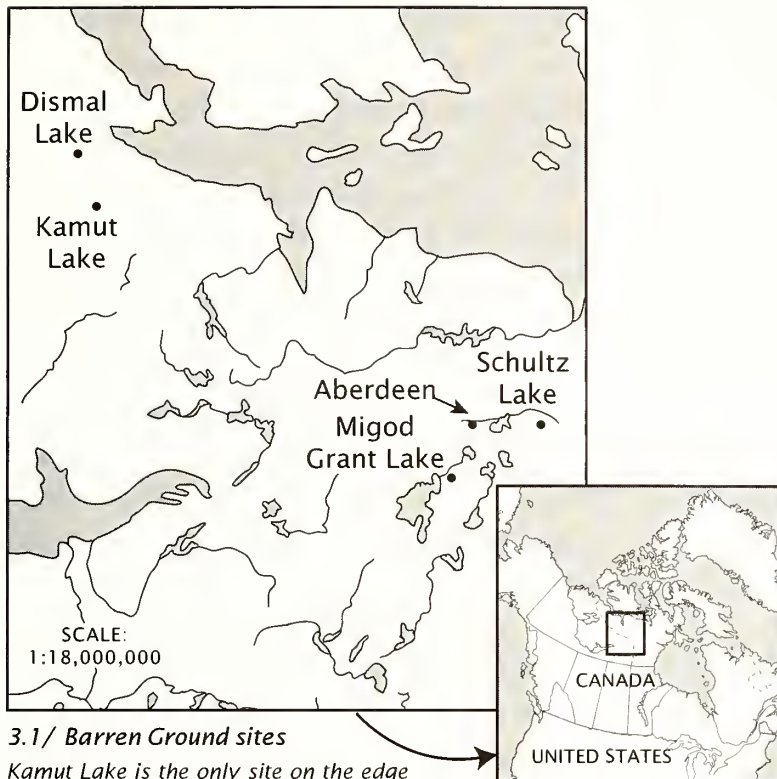
In this chapter, I briefly outline those aspects of Harp's research that had a significant impact on my own research, with its bush rather than arctic orientation. Because of the vast areas involved, ranging from just east of Great Bear Lake in the Mackenzie District of the Northwest Territories to the Barren Grounds of Keewatin District (now mainly part of Nunavut, see fig. 3.1), and then to the east coast of Hudson Bay and southern Labrador, and finally to the Island of Newfoundland, I will arrange my comments culturally and chronologically rather than by geography.

Northern Plano Culture

Following the description of archaeological specimens recovered by the Moffatt canoe party (Harp 1959), Harp conducted an archaeological survey along the Thelon River (Harp 1961), which included a visit to the

important Northern Plano Grant Lake site north of Dubawnt Lake discovered by the Moffatt party. This work provided him with insights relating to the first peoples to penetrate the Barren Grounds. In the conclusion of his Thelon River report, Harp noted that the Thelon River assemblage, which would eventually be classified as Shield Archaic, had probably evolved from the preceding Northern Plano culture (Harp 1961:63).

Harp's interpretation of Shield culture origins was subsequently supported by discoveries in the south (Wright 1972b:69–73). My excavations at Grant Lake (Wright 1976), which were largely stimulated by Harp's site description and collections as well as by an interest in trying to demonstrate technological continuity from Northern Plano to Early Shield culture (Wright 1976), focused on a Northern Plano culture dwelling that Harp had recorded at Schultz Lake on the Thelon River (Harp 1961:18–19). The Schultz Lake site appeared on technological grounds to be late in the Northern Plano culture development and thus became an obvious place for trying to establish continuities from Northern Plano to Early Shield technologies. Because no suitable Early Shield culture assemblages were available in the region for comparative purposes, however, it was not possible to test this idea. But other evidence did add support to the hypothesis (Wright 1976:91–93) and subsequent work at the 8000 B.P. Sinnock site in southeastern Manitoba (Buchner 1981, 1984) produced evidence that further increased the likelihood that Harp's original hypothesis was valid. Indeed, in a recent synthesis of early subarctic and arctic cultures, Harp (1983) saw sufficient continuities to classify what has been referred to here as Northern Plano culture as Shield Archaic. At a time when a Plano culture origin for the Shield Archaic was first being considered, a careful examination of the literature failed to locate a complete description or quantification of a Plano culture component in North America, other than those for the Grant Lake and Schultz Lake Plano sites (Harp 1961). Elmer Harp obviously adhered to the all-too-often-ignored tenet in archaeological reporting that dictates



3.1/ Barren Ground sites

Kamut Lake is the only site on the edge of the present tree line, but all the sites were occasionally within seasonal commuting range of the forest as it fluctuated through time.

that artifacts should be described in sufficient detail to allow future archaeologists to compare such data with newly acquired evidence and thus advance our understanding of past people. It should also be noted that Elmer Harp's admirable sketches of sites and local topography (Harp 1961:fig. 9b) left no confusion about a site's location in the minds of subsequent field workers.

Maritime Culture

Harp's early work in the Strait of Belle Isle region of the north shore of the Gulf of St. Lawrence in Labrador and Quebec and on the Island of Newfoundland (Harp 1951, 1964a, 1964b; Harp and Hughes 1968) revealed the presence of an important maritime-adapted culture that eventually would be classified as the Maritime Archaic (Tuck 1976a). Radiocarbon dates of more than 7,000 years¹ for some of the materials, which at the time were attributed to the Boreal Archaic with correspondences to the Laurentian Archaic of the Upper St.

Lawrence River and Lower Great Lakes, attracted considerable attention and stimulated a number of major research projects along the Labrador coast and on the Island of Newfoundland (Fitzhugh 1972b; McGhee and Tuck 1975; Tuck 1976a). At the same time, researchers working along the Quebec portion of the north shore of the Gulf of St. Lawrence encountered similar remains (Levesque 1980). All of this archaeological activity in the late 1960s and early 1970s led to an increasing number of research programs and eventually resulted in providing the region with one of the best-described archaeological sequences in northeastern North America. Among the archaeological cultures identified, in addition to the Maritime culture, were the Shield, Paleoeskimo, Inuit, and Montagnais/

Beothuk cultures. Related research extended to the interior of Quebec, throughout the Gulf of St. Lawrence, the Maritime provinces, northern Maine, and well up the St. Lawrence River. When Elmer Harp primed the pump, he initiated a massive archaeological artesian outflowing!

Shield Culture

The concept of a Shield culture that was specifically adapted to the special requirements of the Boreal Forest and Lichen Woodland vegetation provinces of the Canadian Shield (Wright 1972a) owes much to the late Frank Ridley's work in northern Ontario and to Elmer Harp's work in the Barren Grounds of Keewatin District and along the southern coast of Labrador and Quebec. The evidence from the Barren Grounds was particularly critical to the concept's formulation. Harp's (1961) Thelon River Complex C materials largely equate with what I would now call Middle Shield culture (4000–1000 B.C.). The Aberdeen site (AL-7), situated along a

bluff on the south bank of the Thelon River, was of particular interest, and I decided to take Harp's advice that "it would repay a planned excavation" (Harp 1961:21). This certainly proved to be the case. Although the cultural debris from multiple seasonal occupations of the site by Northern Plano, Middle Shield, Early Paleoeskimo, and Late Northwest Interior (Taltheilei complex) peoples,² as well as Inuit caribou hunters, were hopelessly mixed in the thin, cryoturbated deposits, the discovery of two Middle Shield culture house structures provided a rare instance of clear component isolation (Wright 1972a). These dwellings not only offered the first glimpse of an important facet of Middle Shield culture settlement patterning but, because the two structures belonged to different periods based on their artifactual contents, they also allowed a seriation of the tool assemblages that provided insights into artifact style and tool category frequency trends for the Middle Shield culture in the Barren Grounds. These trends were found to be similar to those recorded to the southeast in the bush region.

A supplementary result of the large-scale excavations at the Northern Plano culture site at Grant Lake (Wright 1976) was the discovery of the nearby Migod site, situated at the first rapid in the Dubawnt River immediately north of the Grant Lake site. This important stratified site contained occupation levels that represented the entire span of human settlement in the Barren Grounds. Particularly well represented were Shield culture occupations whose sequential assemblages and large numbers of radiocarbon dates have illuminated Shield culture development as well as later developments in the area (Gordon 1976).

Harp's (1964b) discovery of a scatter of Middle Shield culture side-notched projectile points from the north shore of the Gulf of St. Lawrence in the Strait of Belle Isle region heralded the formulation of what eventually would be referred to locally as the Brinex and Charles complexes in Hamilton Inlet in southern Labrador (Fitzhugh 1972b) and the Saunders complex on

the central Labrador coast (Nagle 1978). Related materials have been described from sites in the interior of Quebec (Chevrier 1986) and further up the Gulf of St. Lawrence (Chapdelaine 1984). On the central Labrador coast, the Saunders complex was attributed to a Shield culture intrusion around 2000 B.C. (Nagle 1978:143–144). This population intrusion correlated with the southward push of Early Paleoeskimos down the Labrador coast and the disappearance of the previous Middle Maritime culture inhabitants—cultural events that occurred during a time of climatic change. It now appears that the small sample of materials recovered by Harp from the Blanc Sablon region of the Strait of Belle Isle had a major impact on the development of the culture history of Labrador, as well as of the interior and north shore of the Gulf of St. Lawrence in Quebec.

Northwest Interior and Proto-Northern Athapascan Cultures

Archaeological materials that Harp obtained during the late 1950s from Kamut Lake and Dismal Lake, located between Coronation Gulf on the northeast and Great Bear Lake on the southwest (Harp 1958), and from the Thelon River region of Keewatin District (Harp 1961) can now be placed within a relatively accurate culture historical framework (Gordon 1981; Noble 1971). Despite the limited comparative information available during his early surveys, Harp was able to identify their similarities, such as those reflected by the presence of transverse and corner burins, to the Western Arctic (Harp 1958:238–239). His interpretive perceptiveness—at a time when little comparative evidence was available—is underscored by the fact that, of the six major conclusions that stem from his Thelon River research (Harp 1961:70), only one has not been substantially upheld by more recent work in the Barren Grounds, and that one relates to chronological estimates in lieu of radiocarbon dates and a reliance on apparently erroneous geological estimates of the time of deglaciation.

While I am tempted to include Paleoeskimo in the list of cultures whose formulations and developments have substantially benefitted from Harp's contributions, I will leave such an assessment to the arctic archaeologists contributing to this volume. I would only note that Elmer Harp encountered evidence of Paleoeskimo cultures in all of the northern regions in which he worked and his data have rendered considerable assistance to the understanding of Paleoeskimo penetrations into northern Saskatchewan, Manitoba, Labrador, Newfoundland, and the north shore of the Gulf of St. Lawrence.

It is impossible in a brief note to acknowledge properly the debt that current bush and arctic archaeologists owe to Elmer Harp. In every geographic region in which he worked, he acted as a catalyst for subsequent investigators. His ability to effectively combine accurately described data and insightful interpretations has been responsible for eliciting such a following. While I cannot offhand recall the source, I do remember a number of years ago reading a critique of the "New Archeology" in which it was lamented that many of its advocates seemed to be unable to

stand upon the shoulders of their predecessors without relieving themselves. In this respect, I am clearly not a New Archaeologist, but I deem it to be both a privilege and an honor to have had the opportunity to stand upon the sturdy shoulders of an outstanding scholar like Elmer Harp.

Notes

1. All dates given as either years ago or B.C./A.D. have been calibrated for fluctuations in atmospheric radiocarbon 14 (Klein et al. 1982). If a date exceeded the 7240 B.P. maximum of the dendrochronology-based calibration tables, it is reported in radiocarbon years B.P.

2. Some of the unfamiliar archaeological cultural terms and procedures that appear here, such as the designations Late Northwest Interior culture, the frequent dropping of the qualifier Archaic, and categorizing lengthy cultural traditions (e.g., Early Shield, Middle Shield), are all part of a modified archaeological culture nomenclature. Such changes were required during my writing of *A History of the Native People of Canada* (Wright 1995, 1999, n.d.). Needless to say, the northeastern and northern aspects of this national archaeological synthesis have substantially benefitted from Harp's published contributions.

A History of Beverly Range Archaeological Research

BRYAN C. GORDON

Each spring the caribou of the Beverly population migrate from their winter range in the forests of northern Saskatchewan, moving northeast over frozen lakes and rivers toward their calving grounds next to Beverly Lake in the District of Keewatin, Northwest Territories. The cows drop their calves in high, dry areas, free of predators and insects, before beginning their long migration back to the forests in early July. They join the bulls, forming one great herd of thousands of animals that crosses the Barrenlands between the Thelon and Dubawnt Rivers. Their numbers are so large that the landscape seems to move with them. At water crossings, they fill the rivers and lakes from shore to shore before dispersing onto the tundra over trails worn deep over thousands of years. They have cut the same swath across the Barrenlands twice a year, shifting their route only when climate and overkilling have interfered.

Today, the Inuit hunt the caribou at the northern end of their range, near the calving grounds. The Chipewyan Indians hunt them in the forest at the southern end. In between, a large expanse of land lies uninhabited by hunters at any time of the year. The center of the Beverly Range north of Lake Athabasca and east of Artillery Lake lies in a large area draining toward the northeast to Hudson Bay by the Thelon and Dubawnt Rivers (figs. 4.1, 2). The exposed upper ridges of the Barrenlands are mostly sandstone and granite of the Canadian Shield. The land is laced with eskers—sand and gravel streambeds from rivers that ran beneath the Laurentide ice sheet 10,000 years ago. In places are wind-blown sand deposits and dune fields



that are remnants of glacial lakes. When the caribou are not pressing onward, harassed by insects, they are grazing on the nutritious new sprouts, buds, and flowers found in the large tundra areas covered with sphagnum, herbaceous plants, shrubs, sedges, and grasses.

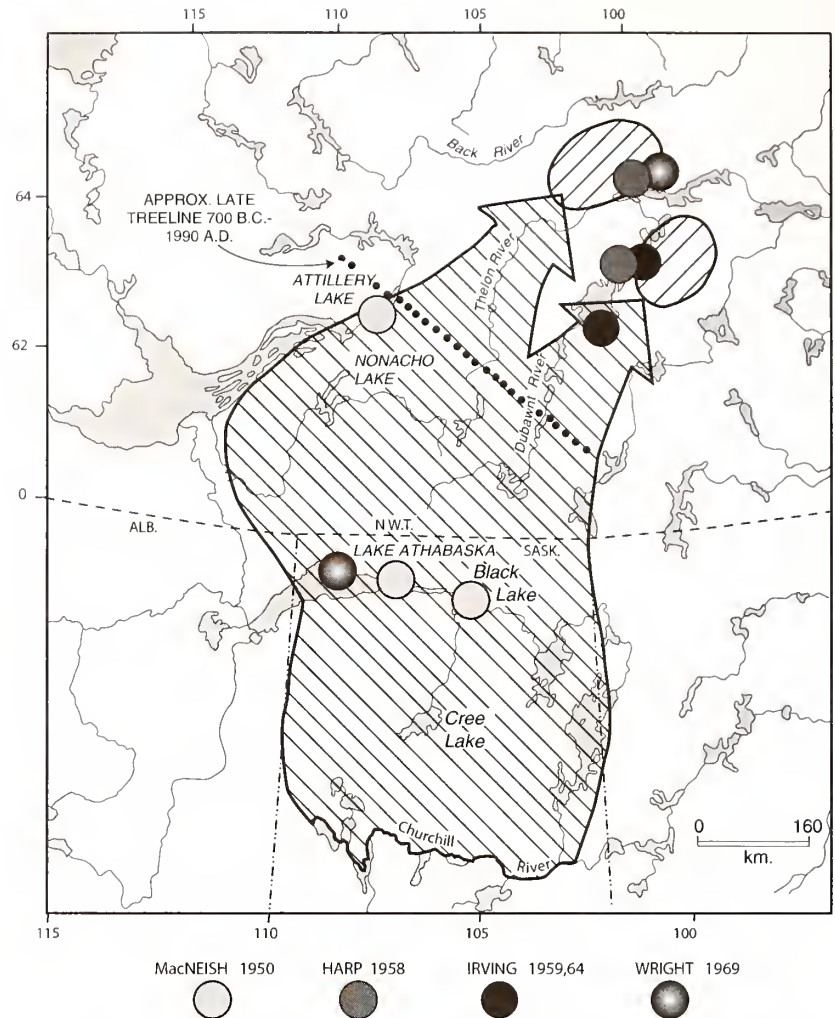
Since the retreat of the glaciers, the region has changed little except for several alternating warm and cool periods. Just as the caribou have responded to a moving tree line as the forest and tundra expanded and contracted, so too did the ancient hunters who followed them. The geographic names reflect this hunting history. Before exploration and the fur trade, the lakes and rivers of the Beverly Range had either Inuit or Chipewyan names, and sometimes both. While *Thelon* comes from the Chipewyan word for “whitefish,” Hanbury (1904:36) reported it as “*Ark-i-linik*” (“wooded river”) of the Inuit, but *Aq-i-liniq* is actually the downriver area near Beverly Lake where the Inuit gathered driftwood. *Dubawnt* in Chipewyan means “ice shore.” *Thlewey-cho-dezeth*, or “great fish river,” might not have been renamed the Back had it been retained by caribou hunters. Today, the geographic names in the Beverly Range are a mosaic of Chipewyan, Cree, French, and English, reflecting a more modern history and obscuring the more ancient one.

European traders in North America were quick to exploit the fur reserves of the northern boreal forest but slow to explore the adjacent tundra. They encouraged Indians to exchange their traditional forest/tundra hunting cycle for the trapping of small animals in the forest. The Indians delivered furs to forest trading posts at locations convenient to the traders. Samuel Hearne (1795:52), who recognized the Chipewyan's dependence on the caribou, noted that they "always follow the lead of the deer [and] are seldom exposed to the griping hand of famine so frequently felt by those who are called the annual traders."

When the Hudson's Bay Company learned that native copper was available on the Coppermine River, it sent Hearne across the Beverly Range to find it. While his odyssey, which lasted from 1769 to 1772, failed to bring profit to the company, his report provided the earliest extensive documentation of traditional Chipewyan life. One of his first camps was at Crow Hill, an esker knob visible for many miles at Grant Lake on the Dubawnt River (Hearne 1795:map).

Expanded Understanding

Our understanding of the ancient peoples who hunted Beverly caribou developed slowly, primarily because of the difficulty of traveling up the turbulent waterways that ran from its heartland. Even the mass movements of different cultures went unnoticed for many years. Sometime between Hearne's visit in 1771 and Tyrrell's visit in 1893, the Chipewyan abandoned Crow Hill and the Barrenlands for the southern fur trade (Tyrrell 1898). In 1954, the geologist John Fyles found ancient



4.1/ Investigations into the prehistory of the Beverly Range area from 1950 to 1969 are shown above. The hatch marks with arrows indicate the path of the caribou herd migration.

Indian tools at Crow Hill, but it was not until 1974, when I excavated, dated, and compared Chipewyan and Inuit tools, that the change at Crow Hill could be dated to about 1815 (Gordon 1976). Before that time, what is now part of Nunavut was Chipewyan.

Later explorers and adventurers, such as Warburton Pike (1892), David Hanbury (1904), and Ernest Seton (1911), also failed to recognize the tools of the ancient Indians and their exploitation of the Beverly Range. Although they commented on this uninhabited wilderness, they did not realize the extent to which it had once been a vital part of the early Chipewyan life cycle.

Northern-oriented archaeologists gradually became curious about who might have lived in this wilderness

and how they lived (table 4.1 , fig. 4.1). But access into the region remained difficult, with the canoe serving as the main conveyance for many years. Richard MacNeish (1951:31) traveled by canoe while collecting material near Artillery and Athabasca Lakes, which helped him define the Artillery, Taltheilei, Lockhart, and Whitefish complexes. On a canoe trip in 1955, Arthur Moffatt found nine sites at Chipman River and at Lakes Selwyn, Boyd, Barlow, and Carey on the Dubawnt River (Harp 1959). Sadly, after upsetting his canoe on a rock in mid-river on a cold day in the middle of September, Moffatt died of exposure onshore (Grinnell 1996). His companions retrieved his collection of artifacts, and these were later studied by Elmer Harp.

After visiting Grant Lake in 1958 and combining this collection with others from Baker, Beverly, and Aberdeen Lakes, Harp defined two Indian Archaic phases—an Early Archaic and a Late Archaic—which were separated in time by the Pre-Dorset. He also noted the presence of a Thule culture precursor to the Caribou Inuit. Later, I was able to divide Harp’s early Archaic into Plano and Shield Archaic and his late Archaic into several Taltheilei phases (Gordon 1975:92–94, 1996). William Irving’s (1968:40–47) subsequent work at Grant and Dubawnt Lakes enhanced our understanding of the Barrenland Pre-Dorset.

In the early 1960s, ornithologist Robert Nero found a number of Besant, Pelican Lake, and Taltheilei points

at Lake Athabasca in the Beverly forest. By using Nero’s information and his own Taltheilei, Pre-Dorset, and Shield Archaic tool collections, Wright (1975) was able to synthesize the prehistory of Lake Athabasca. Further east, Minni’s (1976:158) fieldwork at Black Lake yielded forty-two surface site and thirteen buried sites. The Pre-Dorset endblade she found there marks the southern limit of the Pre-Dorset culture, and a Plano point attests to the 7,000-year-old transition from bison to caribou hunting. To the northwest, Noble’s (1971) survey of Artillery Lake and Pike’s Portage in 1966–1969, although failing to uncover any stratified sites, did advance MacNeish’s Artillery Lake study by sorting out his complexes using Glacial Lake McConnell beach ridges, radiocarbon dates, and typology. In the far northeast, Wright’s (1972a) excavations of a Shield Archaic pithouse at Aberdeen Lake in 1969 revealed the presence of Plano, Pre-Dorset, Inuit, and Taltheilei tools on its surface.

While the central Beverly Range, especially near the upper and middle Thelon River, remained remote, the stage for archaeological investigation was being set (table 4.2, fig. 4.2). After bison were exterminated on the Plains, turn-of-the-century trophy hunters began looking further north for musk ox. One of these, Warburton Pike, helped to simplify canoe travel by publishing the Indian portage route from Great Slave to Artillery Lakes. During their 1924–1925 crossing of

Table 4.1/ Each of the researchers below has contributed to a synthesis of the complex prehistory of the Beverly Range.

Investigator	Year	Contribution and Change
MacNeish (1951)	1950	W. Beverly Range Taltheilei, Whitefish Lockhart, and Artillery Complexes
Harp (1959, 1961)	1958	Early and Late Archaic, Pre-Dorest, Thule in Middle and Lower Thelon; Identified Agate Basin Points
Irving (1968)	1959, 1964	Pre-Dorset on Dubawnt River
Wright (1972, 1975, 1976)	1969, 1973	Shield Archaic Origin, Dated Agate Basin, Lake Athabasca Synthesis
Gordon (1975, 1976)	1970-1978	Excavated and Dated 3 Shield Archaic, 2 Pre-Dorset, 4 Taltheilei Phases; Used 3 Treelines to Separate 1002 Sites

Table 4.2/ Beverly surveys and excavations, by area

Reference	Year	Survey or Excavation
Hearne (1795)	1770	Crow Hill, or Grant Lake Esker
Tyrrell (1898)	1901	Middle Thelon River Survey
MacNeish (1951)	1950	Artillery & Athabasca Survey
Fyles (personal comm)	1954	Reported First Grant Lake Survey
Moffatt (Harp 1959)	1955	Dubawnt River Collecting
Harp (1959, 1961)	1958	Beverly, Aberdeen, Grant Lakes
Irving (1968)	1959, 1964	Grant Lake & Slow River
Wright (1972, 1975, 1976)	1969, 1974	Aberdeen & Grant Lake Excavation
Gordon (1975, 1976)	1970- 1983	Thelon, Taltson, Cree, Hanbury, and Elk Survey and Excavation
Minni (1976)	1972- 1974	Black Lake Survey and Excavation
Kalinka (personal comm), Sharp (1988), Meyer (1979, 1983), Jarvenpa et al. 1988	1970- 1980	Isolated Caribou Inuit and Caribou-Eater Site Finds

Pike's Portage to record musk ox on the Thelon River, John Hornby and Critchell-Bullock collected data that was used by the Advisory Board on Wildlife Protection to establish the Thelon Game Sanctuary (*Canada Gazette* 1927:61:4). In 1929, the RCMP positioned food caches in the middle Thelon in an effort to investigate the starvation deaths of Hornby and two companions who had missed the fall caribou migration. A year later, the RCMP built a cabin in what was later called Warden's Grove to help patrol the new game sanctuary (Hoare 1990). The area, however, remained closed to hunting and mineral exploration. Environmental studies that focused on the region's biology, botany, and archaeology were discouraged by the sheer distance needed to travel there and the burden of allocating half a plane's payload for gas for the return flight.

In the late 1960s, the potential for short-term research in distant areas grew with the availability of air photos and maps, better aircraft and radio transmitters, and light-weight food. The Riveredge Foundation of Calgary launched a two-year field project in the

central Barrenlands through the University of Calgary, after receiving a collection of stone tools in 1969 from Ben Strickland, an amateur archaeologist involved in a program to capture musk ox calves for the Calgary Zoo. The project solved a major logistics problem by using an Armed Forces Hercules to drop several years of supplies at Warden's Grove. Archaeological excavations started in 1971 near the junction of the Hanbury, Clarke, and Thelon Rivers (Gordon 1975) and yielded tools from well-dated, stratified contexts at four large sites. These tools were then used to assign various components from sixty-four nearby surface sites to the Shield Archaic, Pre-Dorset, and Talttheilei phases. Plano period tools were later identified and dated at the Migod site at Crow Hill on the Dubawnt River. Surveys from Warden's Grove were extended downriver to Hornby Point, up the Hanbury River to Dickson Canyon, and overland to Clarke River and Steele Lake. The lower Thelon to Beverly Lake was subsequently surveyed by canoeist and amateur archaeologist Frank Metcalf.

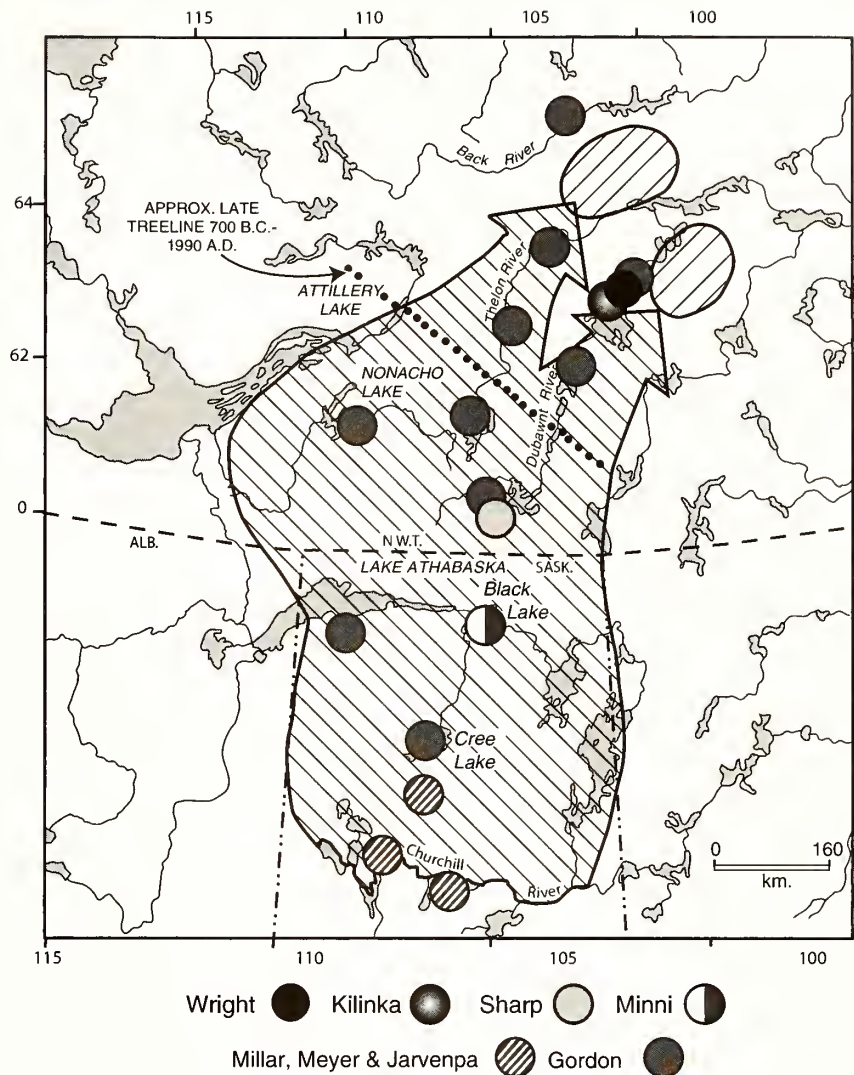
Caribou Ranges

In a comparison of Pre-Dorset tools from the Thelon River area to those from other parts of the Barrenlands, it became clear that caribou ranges were keys to understanding human adaptation. Indeed, the similarity of tools within one range was greater than it was for tools from different ranges. The lives of people and of caribou, the hunters and the hunted, could not be separated. In order to understand the ancient hunters, one had to understand the caribou. Archaeological research became multi-disciplinary, requiring an understanding of caribou habits, ranges, and their influence on hunters now and in ancient times.

The fact that caribou breed separately in fixed ranges (Parker 1972) turned out to be crucial. It helped focus our research on the Beverly range, from the caribou's forested winter range to their calving grounds. Using our knowledge

of herd movements to and from lake and river crossings on the migration route, we carefully examined air photos, thereby minimizing the costs and time of ground survey by developing predictive models of where sites would be located. We analyzed animal bones, as well as the tools used to kill and process the animals, in order to help us compare the present and past migrations of both hunters and animals.

In 1973, we identified the northern limit of the caribou range in a survey of the Back River from McKinley River to Garry Lake, recording Inuit houses, caches, graves, and *inukshuit*. A year later, we recovered tools from all cultural periods at Migod (Gordon 1976). In



4.2/ Archaeological investigations in the Beverly Range from 1970 to 1993 are indicated above, as is the path of the caribou migration.

later years, we located sites in the interior of the range enclosed by the Thelon, Dubawnt, and Taltson Rivers. Because we knew that the Chipewyan and their predecessors, unlike the Cree, used their craft to cross rivers rather than to paddle down them, we were able to confirm, on the basis of interior tundra and tree-line sites, that they had followed the herds rather than the rivers. In 1975, we found Chipewyan tent frames, drying racks, graves, and a family supply box with beadwork and cartridge reloading supplies from the 1930s on the Taltson River, while farther upriver at Gray Lake we discovered a ninety-year-old Chipewyan teepee frame.

Some of the Chipewyan settlements on the Dubawnt headwaters appear to have been completely bypassed by the fur trade and the southward movements of the Caribou Inuit. During our survey, two members of our team camped on one side of a lake. Their barefoot prints, test pitting, and nightly singing terrified the Chipewyan who were on the other side of the lake. The Chipewyan thought the surveyors were *bekaycho*, or bushmen (Sharp 1988); if my crew had not moved on, they might have been shot.

In 1976, the excavation of site KJNb-7 in Warden's Grove provided an early Shield Archaic date of 6050 B.P. The date came from a level associated with a caribou mandible whose tooth increments indicated a spring kill. Because the Shield Archaic climate was warmer, with longer summers, humans may have been able to make earlier seasonal movements.

About 100 kilometers southwest, a survey at a Whitefish Lake water crossing revealed a buried site rich in materials from all Taltheilei phases; this discovery permitted the further classification of tools from surface sites around Lakes Jim, Mantic, and Sid and down the Elk River to Warden's Grove. A 4,200-year-old Duncan point found in a level contemporaneous with the Shield Archaic appeared to have been brought from the forest by Archaic hunters. Tools from all Taltheilei phases were found at Cree Lake in northern Saskatchewan. Henry Sharp (personal communication 1993) discovered some Chipewyan sites located away from the shore of Firedrake Lake, which we had missed in our 1977 survey. In the 1970s, Meyer (1979, 1983) found evidence of Cree and Late Taltheilei contact on the Churchill and Haultain Rivers of northern Saskatchewan.

To Wright's and Nero's site inventories from Lake Athabasca, we added sites between the William River and Yakow Lake and also dated a buried Chipewyan level. Further east at Fond-du-Lac, we continued MacNeish's earlier survey, locating artifacts and chipping stations that were visible in large blowouts on the south shore. Most of the artifacts were Taltheilei, but some

small chert flakes were reminiscent of Pre-Dorset. The excavations at Mosquito Lake in 1982 and 1983 revealed tools from the Taltheilei and Pre-Dorset periods. Plano points were curiously absent even though 134 of them had been found at Grant Lake 100 kilometers to the northeast.

In 1977, Hans Kalinka (personal communication, 1978) found a number of historic Caribou Inuit sites at Aberdeen Lake where the Thelon and Dubawnt Rivers merge. A year later, he uncovered a whale bone sled runner at the entrance to Grant Lake, along with a site (KkLn-18) at the Chamberlin River. Tom Foess (personal communication, 1984) recovered a long, Chipewyan metal lancehead at KcNe-1 below Whitefish Lake. Rob Common, while wintering at Warden's Grove, found a Middle Taltheilei point at KkNb-23, near the remains of an incinerated Soviet nuclear-powered satellite (ASC Archives). In the 1980s, Robert Jarvenpa and Hetty Jo Brumbach located Chipewyan sites between Cree Lake and the Churchill River.

While Hearne had confirmed the historic use of the central Beverly Range by the Chipewyan, recent archaeological researchers have recorded the presence of the historic tundra Inuit, who had derived from the coastal Thule. After the Chipewyan abandoned the tundra during the early fur trade, the Inuit had expanded their hunting area southward. Over the years, however, the Inuit adaptation proved unsuccessful because they were confined to the tundra and were prevented, by the historic forest Chipewyan, from following the herds south.

Beverly Range Chronology

Since 1970, the discovery of many new sites has enhanced our understanding of the early inhabitants of the Beverly Range. The large body of artifacts, sites, and dates has allowed us to divide the early history of the Beverly Range into several traditions, or past ways of life. These, in turn, have been subdivided into different phases based on changing toolkits (table 4.3). More than 100 radiocarbon dates have confirmed the con-

Table 4.3/ Beverly Range chronology and respective investigators for each period

Tradition/ Phase	Period (Years Ago)	Investigators
Caribou Inuit	Historic	Gordon, Harp, Kalinka, Tyrrell,
Chipewyan	Historic	Gordon, Hearne, Jarvenpa, Sharp, Tyrrell
Late, Middle, Early, and Earliest Taltheilei	200–1300, 1300–1800, 1800–2450, 2450–2600	Gordon, Meyer, Wright
Late and Early Pre-Dorset	2650–2950, 2950–3450	Gordon, Harp, Irving, Minni, Wright
Late, Middle, and Early Shield Archaic	3500–4450, 4450–5450, 5500–6500	Gordon, Harp, Irving, Wright
Northern Plano	7000–8000	Gordon, Harp, Irving, Minni, Wright

tinuous human occupation of the region through four major traditions: Taltheilei (Historic–2600 B.P.), Pre-Dorset (2650–3450 B.P.), Shield Archaic (3500–6450 B.P.), and Northern Plano (7000–8000 B.P.).

The Late Taltheilei phase merges with historic forest Chipewyan. Late Taltheilei/Chipewyan sites feature European goods that were brought inland from Hudson Bay about 300 years ago, well ahead of the fur traders. Iron, steel, brass, copper, cloth, pottery, and glass items were exchanged with other Indian groups for fur and meat. These items occur among collapsed tent poles and tent rings and are mixed with quartzite tools in upper archaeological levels. These sites are too recent for radiocarbon dating. Missing from the archaeological record, however, are perishable items, such as crude wood paintings, quill work, moose-hair embroidery, double paddles borrowed from the Caribou Inuit, and birchbark boiling baskets from the Cree. These objects were described by Hearne (1795) during his early visits to the tundra and forest areas.

The Chipewyan emerged from the archaeologically identifiable Late Taltheilei phase (200–1300 B.P.). This phase is characterized by small notched arrowheads, asymmetric tools, and crude unpatterned bone and wooden tools. The projectile points vary more than they do in earlier phases, primarily because of the addition of the bow-and-arrow to an earlier technology based on lancing caribou at water crossings.

In a half-dozen stratified sites, the Late Taltheilei levels are underlain by Middle Taltheilei (1300–1800 B.P.) levels. The Middle Taltheilei is known for its

standardized long-stemmed lanceheads and knives used to spear and butcher caribou at water crossings along the migration corridor. It has more identifiable knives than other phases, as well as triangular scrapers. The Early Taltheilei phase (1800–2550 B.P.) is characterized by shouldered points and knives, which change gradually from dual to single shoulders and then to a stem as time passes.

The Pre-Dorset peoples of the Arctic Small Tool tradition (3450–2650 B.P.) represent an intrusion of Inuit-related peoples that can be traced to the Siberian Neolithic (Irving 1970:341). The Pre-Dorset period began more than 4,000 years ago in the High Arctic, but after 500 years many hunters moved south when extreme cold curtailed their maritime hunting activities. These hunters occupied the Barrenlands until 2,650 years ago, quickly adopting the practice of following the herd, and adapting to both forest and tundra environments. Because only a tenth of the caribou winter on the tundra, making hunting perilous, most hunters stayed near the tree line. When the climate warmed, the Pre-Dorset peoples returned to the coast.

The Shield Archaic peoples occupied the Beverly range during the “climatic optimum,” or Hypsithermal (6500–3500 B.P.). Wright (1976:91–93) has suggested that they developed from the Northern Plano based on a change from lanceolate to long, elegant side-notched points and on similar types and ratios of burinated points and unifacial knives. The sites Wright excavated at Grant, Aberdeen, and Schultz Lake Plano, however, have no Early Shield Archaic components,

and thus provide no data to confirm or deny his transition hypothesis. An Early Shield Archaic point dating to just prior to the Plano phase at Warden's Grove, however, is short and crude, suggesting that further fieldwork is needed to resolve the issue.

The oldest tradition in the Barrenlands is Northern Plano, characterized by Agate Basin points. These points are 9,000 to 10,000 years old at their type site in Wyoming. Midway through the Prairies, they are 8,000 to 9,000 years old. In the Barrenlands, they range from 7,000 to 8,000 years old. Plano bison hunting camps are found along a thin line extending from Wyoming to northern Saskatchewan, but at Lakes Athabasca and Black, caribou become the predominant prey.

While the general sequence of Beverly range phases is well known, some of the details (e.g., the origins of the Shield Archaic) are still unclear. Current studies have begun to identify the differences between forest and tundra artifacts. Lithic artifacts from the forest, for example, tend to be smaller in size because they were worn or resharpened repeatedly. The sources of stone sources were far away on the tundra or in the forest under snow cover. The styles also are different; the knife hafts found at forest sites, for example, are tapered to allow them to be inserted, with mittened hands, in the marrow cavity of a long-bone.

Conclusion

Undoubtedly, the caribou influenced every aspect of life for those who depended on them. Their availability

and physical condition greatly influenced human nutrition and birth cycles. Baptismal certificates since the 1850s show that four out of five Chipewyan births occurred in February, March, or April, nine months after the fall caribou migration. In the fall, human nutrition, especially in terms of fat consumption, would have been greatest, and women would have been most fertile (i.e., when fat exceeded 12 percent of body weight). As I have noted elsewhere, "the Chipewyan cycle of July–August conception and March–April birthing meshes well with the caribou cycle, just as it undoubtedly did for earlier peoples" (Gordon 1996:16).

The study of animal ranges and migration routes to discover site locations and interpret artifacts can be applied to areas outside of the Barrenlands. I have used the principles learned in the Canadian Arctic to investigate past herd following in northern Russia, where I found differences in tools and materials between tundra and forest ranges and between seasons (Gordon 1998, 2000).

Archaeology is built upon a framework developed through many years of fieldwork and research. In particular, the pioneers of archaeology in the Beverly range—Elmer Harp, Richard MacNeish, William Irving, and James Wright—followed their curiosity and their instincts to provide this framework. We honor them by continuing what they began, by working with integrity, by analyzing well, and by holding on to the spirit of discovery that guided them.

Diamond Jenness: The First Canadian Arctic Archaeologist

DAVID MORRISON

Well over half of the North American Arctic is Canadian territory, yet there was only one Canadian in the first generation of arctic archaeologists, a group otherwise dominated by Danes and a few Americans. The lone Canadian was Diamond Jenness. Even here, Canada's claim is not complete because Jenness was born in New Zealand in 1886 and educated there and at Balliol College, Oxford.

But Canada's claim is still a strong one. After joining the Canadian Arctic Expedition in 1913, Jenness lived the rest of his life in Canada until his death near Ottawa in 1969. Between 1916 and 1918, he served with the Canadian Army in France, returning to take a position with the nascent National Museum of Canada (fig. 5.1). In 1926, he replaced Edward Sapir as chief anthropologist at the museum, a position he held until his retirement in the late 1940s. In 1937, he was elected president of the Society for American Archaeology and, in 1939, president of the American Anthropological Association, the only Canadian to have held both honors. He also became a Companion of the Order of Canada in the year of his death (Collins and Taylor 1970; Richling 1990).

Jenness denied any status as an archaeologist. Certainly he is best known for his ethnographic work, particularly among the Copper Inuit of Coronation Gulf (1922), and the publication of his classic *Indians of Canada* (1932). My claim for him as Canada's founding arctic archaeologist rests on three major achievements: his work at Barter Island, his identification of Dorset culture, and the basic Eskimo cultural sequence he defined as Bering Strait.



Barter Island

Jenness conducted the first scientific archaeological work in the North American Arctic. This was his excavation at Barter Island in 1914, which he undertook while creatively "killing time" with the Canadian Arctic Expedition (Jenness 1957, 1990; Hall 1987). Although credit for the first arctic excavations sometimes goes to Vilhjalmur Stefansson because of his work a few years earlier at Point Barrow, Alaska, and at Franklin Bay, in western Arctic Canada (Morrison 1990; Stefansson 1913a), Stefansson was no archaeologist. He kept a few notes but made no maps, floor plans, or photographic records. Like Captain George Comer in the Eastern Arctic, he was a collector. Almost all of the extensive archaeological collections attributed to him in various museums, including the Canadian Museum of Civilization and the American Museum of Natural History, were purchased through the fur trader Charlie Brower from Alaskan Inupiaq "subsistence diggers" (the term is borrowed from Staley 1993).

Of course, a coherent history of arctic archaeology in North America usually starts with neither Stefansson nor Jenness but with Therkel Mathiasen and his Thule culture excavations around northern Hudson Bay in the

early 1920s (Mathiassen 1927b). This is entirely appropriate. Jenness did not publish his Barter Island material until 1957 and then in a rather narrative format. In fact, the material was not fully described until Edwin Hall wrote it up in his report “A Land Full of People, a Long Time Ago” (Hall 1987), and even that remains unpublished. Nonetheless, the claim that Jenness undertook the first scientific excavations is a solid one. He made detailed notes, took photographs, drew house and floor plans, and knew where his artifacts came from. As Hall (1987:18) remarks,

Jenness

“took a scientific approach to the process of archaeological excavation. He was concerned about preserving the integrity of the archaeological remains by applying scientific methods . . . to the excavation process. He was innovative in attempting to defeat the ground frost problem . . . [and] relatively rigorous in the documentation of sites, features and artifacts.”

Jenness, in other words, set a standard for archaeological field techniques that was not eclipsed until the 1950s.

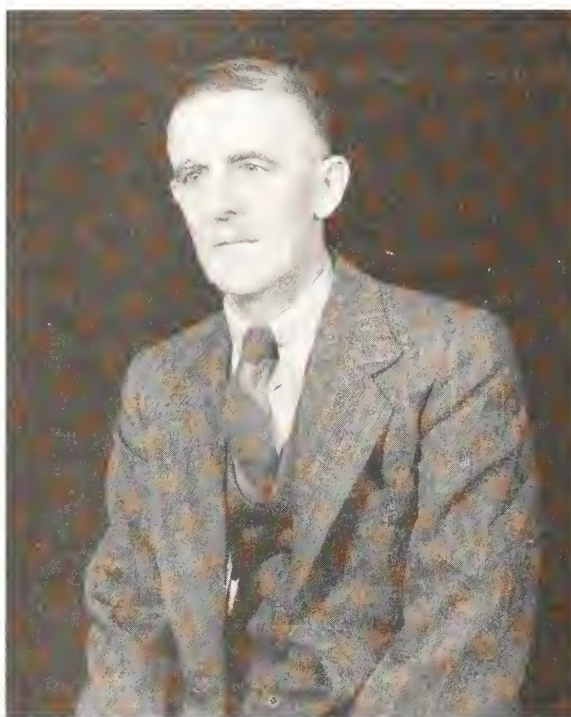
Dorset Culture

Jenness’ second crown of laurels is perhaps the best known: his identification of Dorset culture in 1925 (Jenness 1925). He did this on the basis of a mixed collection donated to the National Museum of Canada by a government engineer named L. T. Burwash. Part of the collection was said to have come from somewhere near Cape Dorset on southern Baffin Island—the site has yet to be identified—and the rest from nearby Coats

Island. Jenness was able to sort out familiar Thule and more recent Inuit artifacts from among a number of unfamiliar, and apparently older, tools. He ascribed these tools to a distinct “Cape Dorset culture,” a feat of deduction that Collins and Taylor (1970) later described as “one of the most brilliant in the history of Arctic archaeology.”

When Jenness finally published his “Archaeology of the Central Eskimo” two years later, Mathiassen took issue with his conclusions. Mathiassen believed that

Thule culture—his Thule culture—was the basic substratum of arctic history and nothing could be older than Thule. His famous debate with Birket-Smith over the Paleoeskimo, or even Protoeskimo, status of the inland Caribou Inuit living west of Hudson Bay can be seen in this light (Birket-Smith 1930; Mathiassen 1930b) and so, too, can his denial of Dorset culture as a discrete and earlier cultural entity (Mathiassen 1927b: 164–165). Of course, while Mathiassen was right with Birket-Smith, he was wrong with Jenness.



5.1/ Diamond Jenness initiated archeological work in the North American Arctic.

Bering Strait

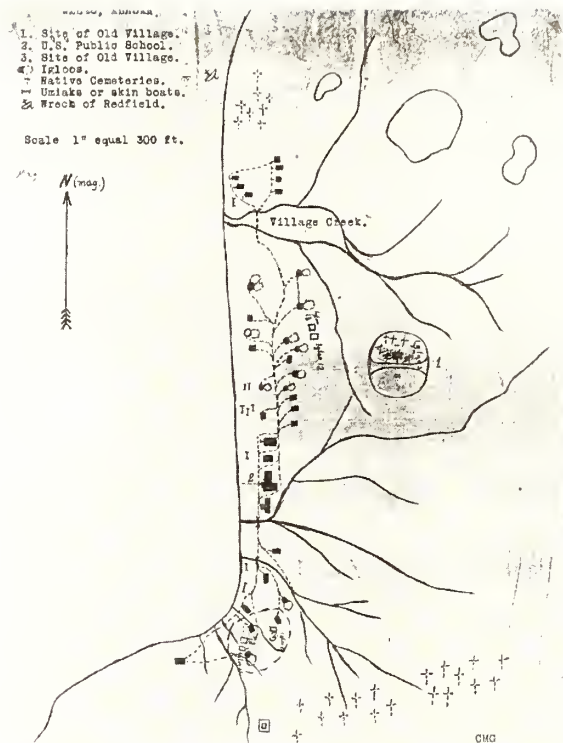
Jenness’s third claim to fame as an archaeologist is the most substantial. This is his work in the Bering Strait region in 1926 (Jenness 1928a; Morrison 1991). Even at this early time in the history of archaeology, some Canadian Arctic questions could be answered in Alaska, and it was Jenness who first looked for and found them.

Jenness went to Bering Strait with the stated purpose of investigating the origins of “Eskimo” culture in order to determine “whether it arose in Alaska or

elsewhere" (Jenness 1928a:71). He worked at two locations: Cape Prince of Wales, at the tip of the Seward Peninsula, and Little Diomed Island, in the middle of Bering Strait. His analysis of this work was limited to a short preliminary report (Jenness 1928a) and a few subsequent discussions elsewhere (Jenness 1929a, 1933), perhaps because he had been appointed chief anthropologist of the National Museum of Canada the same year he went to Alaska and had new demands on his time (see Richling 1995). Together, however, these writings offered observations and conclusions that were at least as profound as those in his 1925 Dorset paper.

At Cape Prince of Wales, Jenness concentrated on a large mound located just behind the modern village. He never gave it a name beyond the designation "Old Village" (fig. 5.2), but it seems to have been the same mound that Henry Collins later excavated, called Kurigitavik (Collins 1937a, 1941). The mound was pitted with house ruins, and Jenness, with the help of an elderly local man and half a dozen Boy Scouts, was able to excavate eight of them (fig 5.3). Elsewhere in the village, he excavated four other houses. Altogether, it was a very impressive number, since he spent only thirty-five days at Wales and had other work to do.

Jenness collected about 1,800 artifacts from Cape Prince of Wales, and they are still part of the archaeological collections of the Canadian Museum of Civilization (Old Catalogue System IX-F-6678-8497). He divided the ruins into two main groups on the basis of their artifact inventories. Several house ruins located around the village were evidently post-contact in age. They produced harpoon heads with iron endblades, iron pipe cleaners, and an occasional glass bead. These trade goods were rare enough, however, to reflect an early contact situation. The houses in the Kurigitavik mound, however, were clearly older. None of them yielded Russian trade material. A pair of superimposed houses situated on a bank overlooking the mound was also pre-contact in age.



5.2/ Jenness' sketch map from his 1926 excavations at Cape Prince of Wales, Alaska.

In addition to the presence or absence of Russian trade goods, Jenness focused on harpoon heads as a key variable for seriation. He noticed that the post-contact houses yielded primarily a modern type of closed-socket harpoon head, along with a few open-socket specimens with drilled lashing holes. The open-socket forms were identical to the Thule culture harpoon heads, specifically Thule type 3, which Mathiassen had recovered from Nauyas and other Canadian sites (Mathiassen 1927b:18-20). Apparently such harpoon heads had not only been distributed throughout the North American Arctic but had also persisted in use at Bering Strait until only a few centuries ago.

Many of the houses in the Kurigitavik mound and the two superimposed houses on the bank seemed to document a purer, or older, form of Thule culture. The harpoon heads from these houses were an open-socket Thule type, again mostly Thule type 3 and variants thereof, but with lashing slots rather than drilled holes. Jenness was able to identify a clear cultural sequence that ran from Thule to recent times, and a

three-part harpoon head sequence that extended from Thule open-socket with slotted lashing holes (fig. 5.4d), to Thule open-socket with drilled holes (fig. 5.4b,c), and finally to the modern closed-socket types (fig. 5.4a).

After a month at Cape Prince of Wales, Jenness hitched a ride to Little Diomed Island where he excavated several more contact-period houses and a midden that extended into pre-contact times. Everything that he found reinforced the sequence that he had already established at Cape Prince of Wales. At the same time, he purchased some artifacts of previously unknown types from local native diggers. Made of ivory, these artifacts were dark and heavily patinated and beautifully decorated with swirling incised lines. The harpoon heads were unlike anything he had seen before, with inserted flint sideblades and multiple basal spurs. One such harpoon head, which Jenness saw being recovered by someone digging an ice cellar, came from a depth of 8 feet (2.5 meters), far deeper than the more familiar material. Evidently, the culture that had produced these beautiful tools was much older than Thule. Jenness named it the Bering Sea culture; today, it is known as Old Bering Sea.

Jenness made three important observations about this newly expanded cultural sequence for western Alaska (Jenness 1928a, 1933). First, he suggested that the Bering Sea culture was about 2,000 years old and had mainly a Bering Sea distribution—ideas that are essentially correct (see Gerlach and Mason 1992). He also proposed that this culture's influence extended as far northeast as Point Barrow, referring here to Stefansson's Birnirk collection, which at first he failed to clearly distinguish from Old Bering Sea proper (Jenness's Bering Sea collection from Little Diomed Island includes a number of Birnirk types). Finally, he saw the Old Bering Sea culture as ancestral to Thule.

Of course, not all of Jenness' observations were new. Mathiassen (1927b:182–184) had suggested an Alaskan origin for Thule, noting strong similarities between Canadian Thule culture and the traditional culture of northwest Alaskan Inupiat. This suggestion was strengthened by the identification of Thule-type harpoon heads in the Alaskan archaeological collections made by Stefansson, Ras-mussen, and others (see Mathiassen 1930a; Wissler 1916). But Jenness receives the credit for actually demonstrating this suggestion. First, Jenness actually excavated several Thule culture sites in Alaska; these were not just a few stray finds out of context but coherent artifact assemblages with associated features and faunal material. Second, and most importantly, he placed the Thule culture into an archaeological framework; he showed stratigraphically how it underlaid the modern culture and, in turn, was underlain by the more ancient Bering Sea culture in a clear and more or less continuous cultural sequence.

It is interesting to note that Mathiassen once again disputed Jenness's conclusions. In *Archaeological Collections from the Western Eskimos*, Mathiassen (1930a:78) denied that Birnirk and Old Bering Sea were ancestral to, or earlier than, Thule culture. For



5.3/ With the help of an elderly assistant (above) and half a dozen Boy Scouts, Jenness excavated twelve houses at Cape Prince of Wales.

Mathiassen, Birnirk was a cross between Old Bering Sea and Thule, a kind of transitional culture that documented a short-lived Bering Sea influence along the Arctic coast. This influence waned as more Thule immigrants arrived, possibly from Asia. For Mathias-sen, Birnirk only appeared to be older than Thule, implying that Bering Sea might not have been an “Eskimo” culture at all. We can see Mathiassen once again attempting to maintain the priority of Thule culture in the face of Jenness’ evidence to the contrary. Mathiassen may have felt that his explanation was a little too ingenious because he concluded his argument with a near-disclaimer: “The conditions seem complicated, and the explanation given will possibly be rejected later on when some day the so badly needed archaeological investigations in the Bering Strait region are undertaken.”

But the investigations had already been undertaken. The answers to Mathiassen’s questions were already apparent in Jenness’ conclusions. Certainly, they were conclusively verified by Henry Collins’ (1937a, 1937b, 1941) subsequent excavations.

Conclusions

The first systematic archaeological work in the Arctic during the early decades of the twentieth century produced two general models of culture history. One was a simple model, espoused by Mathiassen (1927b, 1930a, 1930b), that subsumed nearly everything within Thule culture. The second, more complex model was suggested by Jenness from the perspective of his re-

search in both the Western and the Eastern Arctic. In his brief discussions of the Old Bering Sea culture, Jenness (1928a, 1933) stressed its relative sophistication, both artistically and in other realms. But even this culture, the earliest Western Arctic culture then known, could not be the Protoeskimo culture that the eth-

nologist Steensby (1917) and his disciples posited. Jenness (1925:437) made a similar point in his discussion of the Dorset culture, which he said was “certainly not the culture of the first Eskimos who settled on the coast and gained their livelihood by hunting sea mammals. Of that earliest culture we have yet to find the remains.” The cultural history of the Arctic, indeed, must be complicated to encompass two such early, dissimilar, and already-sophisticated cul-

tures as Dorset and Old Bering Sea. And so it has proved to be.

It is a pity Jenness never thought of himself as an archaeologist. The arctic sequence as we now understand it was already implicit in his conclusions seventy-five years ago.

Acknowledgments

I wish to thank Susan Rowley for her help in locating Jenness’s 1926 field notes, including the site map reproduced here as Figure 5.2, from among Henry Collins’ papers at the Smithsonian Institution. I am also grateful to Ian Dyck for editorial assistance, William Fitzhugh and Stephen Loring for their invitation to attend the Elders Conference, and Dan Odess for rescuing the conference proceedings, and this paper, from oblivion.



5.4/ Bering Strait harpoon-head sequence



(L to R) Norman Emerson, William Taylor and Henry Collins at Native Point, Southampton Island, 1954

part 2

HIGH ARCTIC TRAVEL, PHILOSOPHY, AND THEORY

Tradition and Continuity in Eskimo Art

EDMUND CARPENTER

This chapter is based largely on the research of Carl Schuster (1904–1969). A more extended treatment of his general thesis as well as details about the figures in this chapter¹ can be found in Schuster and Carpenter's *Social Symbolism in Ancient and Tribal Art, 1986–1988*. This massive, twelve-volume work was privately printed and copies were deposited in 600 academic libraries (see also Schuster and Carpenter 1996).

Schuster employed the comparative method to trace a memory link from yesterday back to Paleolithic times. He did so in the face of professional skepticism. Henri Breuil warned him: "Everytime is mixing thing from quite different countries one will arrive to nothing!" (personal communication).

In 1955, Schuster submitted for publication a study of Paleolithic traditions in Eskimo art, but it was rejected on the advice of an editorial reader who wrote that "the premise that prehistoric art can be interpreted by modern primitive art is out of date."

Fashion aside, tribal arts offer a valuable—perhaps the only—means of penetrating certain areas of ancient art, hitherto terra incognita. Schematic art of prehistoric times will remain a subject of futile speculation as long as it is *not* placed on a comparative basis with modern tribal designs. The basis for this approach is simple: art begets art; if you seek the wellspring of traditional art, be prepared to dig deep.

Upside-Down Ladies, Birds, and Animals

Paleolithic and Eskimo artists produced three types of inverted images: humans, birds, and animals. Human



figurines from France (fig. 6.1), European Russia (fig. 6.2), and the Late Paleolithic site of Malta in central Siberia (figs. 6.3, 4) were designed to hang upside down. Although not all Paleolithic figurines were perforated in this way, many were, and some that were not may have been attached by adhesion. Suspending them from their feet, presumably on necklaces or as single pendants, was clearly an established custom throughout wide areas of Eurasia, from Aurignacian through Magdalenian times. This practice continued into later eras in Eastern Europe, the Near East, Polynesia, Indonesia, and especially in arctic Canada-Greenland. The figurines shared more than an inverted suspension. Facial features were generally omitted, though not always. Arms were minimal or absent. Legs tapered to a common point. Buttocks suggested steatopygia.

Most Canadian-Greenlandic figurines (figs. 6.5–8) belong to the Thule culture (ca. A.D. 900–1500), a relatively late phase of Eskimo prehistory. One especially fine example (fig. 6.9), however, appears to be the product of the antecedent Dorset culture, which flourished about 800 B.C.–A.D. 1300 in these regions.

Whether these objects were worn singly or alone, I do not know, but a small excavation near Igloodik produced several that were alike, raising the possibility that they were worn together. Moreover, pendants with two or even three inverted figures (fig. 6.8) look like excerpts from necklaces, i.e., taken from a necklace pendant series.

Eleven pendants of lignite (fig. 6.10) from southern Germany, dating to the Terminal Magdalenian (ca. 8000 B.C.), were found together, suggesting they formed a necklace. The larger pendants were perforated. The smaller ones were presumably attached by adhesion, an explanation that may also apply to the tiny, unperforated female figurines from the Paleolithic site of Mezin in European Russia. The Magdalenian lignite pendants have an expansion just below each perforation, on the same side as the buttocks that must represent the calves, which suggests that these figures hung upside down.

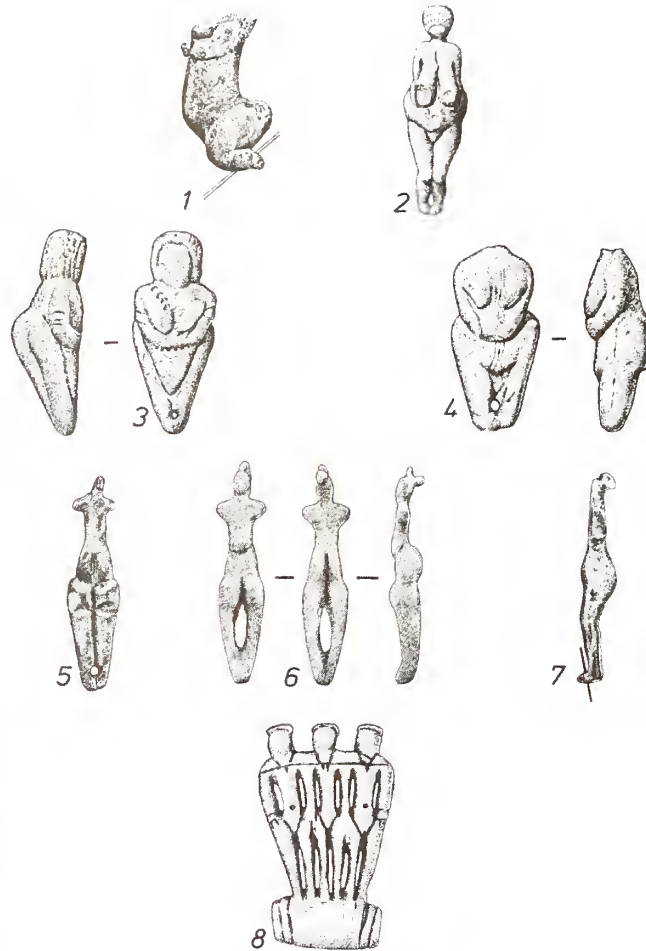
Clearly the inverted-female pendant, singly or in graduated series on a necklace, persisted for many thousands of years during Paleolithic times and into later eras. Eskimo examples originated, I believe, in the Old World Paleolithic, not in Neolithic times, and survived in the Far North until yesterday.

The prominent feature of all Paleolithic images of women is steatopygia (fig. 6.11). Paleolithic artists presumably imitated nature. Eskimo artists presumably

imitated art, for steatopygia is not characteristic of modern Eskimos. Some Thule artists retained the steatopygous form by emphasizing boots (fig. 6.5). Others simply endowed their models amply or ignored this feature. Compare, for example, figure 6.12, a Paleo-

lithic figurine from Italy, with figure 6.9, the Dorset figurine from Canada. Such images survived into Mesolithic, Neolithic and Eneolithic times in Europe and the Near East. An Early Neolithic necklace (fig. 6.13), from Çatal Hüyük in Anatolia (7th millennium B.C.), when carefully examined, reveals an image of steatopygous buttocks perforated for inverted suspension. Debased pendants resemble but, I believe, do not represent birds.

Ancient carvers, as copyists, may themselves have fallen into this misinterpretation. I see this error as the origin of Eskimo images of



6.1 through 6.8/ Human figurines from the Eurasian Paleolithic (1-4) and Canadian-Greenlandic Thule Culture (5-8)

swimming ducks (fig. 6.14), some of which had human busts (fig. 6.15). Both types were popular from Alaska to Greenland. Eskimos used them in *tingmiujaq*, a throwing game of chance. They often perforated each bird at its tail and then, to store them, strung them on a cord, hanging them head down. Strung sets resembled necklaces of inverted-female images (fig. 6.16).

Not all inverted-bird images had this origin. Some were clearly intended as bird pendants: an example from Paleolithic Malta in central Siberia (fig. 6.17) and

from a Dorset site in eastern Canada (fig. 6.18). Dorset bird pendants almost uniformly hung upside down. So did Dorset animal pendants (fig. 6.19). Inverted animal pendants also occurred in the European Paleolithic (fig. 6.20), with a well-worn hole through the rear leg. In other words, Paleolithic and Eskimo artists inverted three effigy types: human, bird, and animal. They also shared the bilobed bead or pendant. If we can judge from related evidence, specimens from the European Paleolithic, including material derived from East Gravettian culture (ca. 24,000 B.C.) in Moravia, represented a woman reduced to breasts (fig. 6.21). Graduated series of such beads were strung on necklaces in both Paleolithic and later times. In Eskimo art, bilobed pendants ranged from Alaska to Greenland. One form resembled firm breasts (fig 6.22); another, slumped breasts (fig. 6.23). Eskimos also shared

with Paleolithic peoples the single-lobed pendant, flattened on one side. When joined, two formed a bilobed pendant.

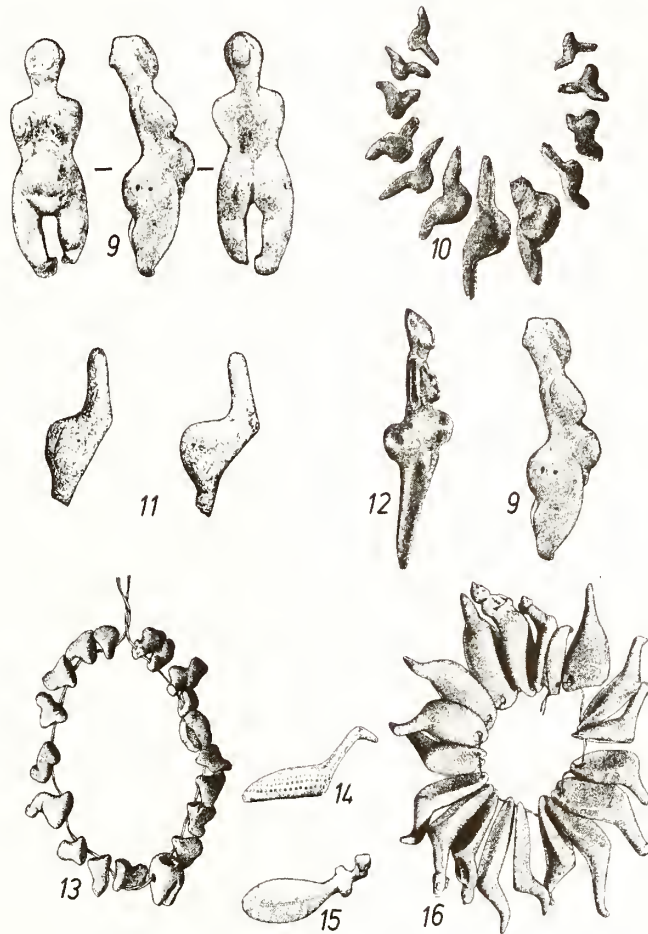
Segmented pendants were also common to both Old and New Worlds. I know of no identifiable Paleolithic examples, but Neolithic examples are so common and so widespread that Paleolithic examples may simply await discovery. Or, perhaps, segmented pendants did not join this assemblage until Neolithic times. In any event, they were present in the Arctic and elsewhere.

Precisely the same inventory of inverted-human, inverted-bird, inverted-animal, bilobed, and segmented pendants occurred in Polynesia. All of them, save the inverted-human figure, graced single neck-

laces on Mangaia, and that one exception occurred elsewhere in Polynesia, both in naturalistic and in abstract form. Neighboring Borneo had entire necklaces of inverted-human figures, including "abstract" steatopygous examples. Precisely the same pendant forms also occurred in the Neolithic and Eneolithic Near East. As in the Arctic and Oceania, several forms frequently appeared together on single necklaces, especially necklaces with inverted-female pendants.

I see inversion as a means of indicating that figurines repre-

sented dead persons, or ancestors. I think they were worn to invoke ancestral powers to protect the living. Such effigies were not confined to necklaces. On Saint Lawrence Island in the Bering Strait, along the Amur River in eastern Siberia, on Queen Charlotte Islands in British Columbia, and in New Guinea, inverted-human effigies surrounded houses. Whether around a necklace or house, the symbolism remained unchanged: a protective border of ancestors guarding the living, sometimes encircling a human neck, sometimes encircling a ceremonial house.



6.9 through 6.16/ Pendants and necklaces of inverted females (9-12) may have evolved into Eskimo swimming duck figurines and pendants (13-16).

Other Motifs

While much more evidence of this curious tradition exists, our subject here concerns Paleolithic elements in Eskimo art—and we've only begun. There is, for instance, a rear flap on the coat of the Aurignacian "Venus" of L'espugue (fig. 6.24). There is also an Aurignacian notched pendant resembling a bull-roarer (fig. 6.25). Other examples of what appear to be bull-roarers occur in the Old World Paleolithic and Mesolithic. Among modern tribes, bull-roarers occur in Asia, Africa, Australia, Oceania, North America, and also among English schoolboys. Eskimos, from Alaska to Greenland, made them, many of which they notched. They also made buzzers, both the notched-disc type, which are so common elsewhere, and the rarer hourglass type. Among many modern tribesmen, the hourglass motif represents an ancestress.

Seams

In Western design tradition, garment seams are a necessary evil, to be "thought away" as irrelevant to the design itself. One might suspect they would be suppressed everywhere in reproduction. But in ancient representations of clothing they are retained, even deliberately emphasized. Seams that joined panels blazoned with "genealogies" of schematic human figures symbolized marital unions. It was this message that ancient artists wished to convey.

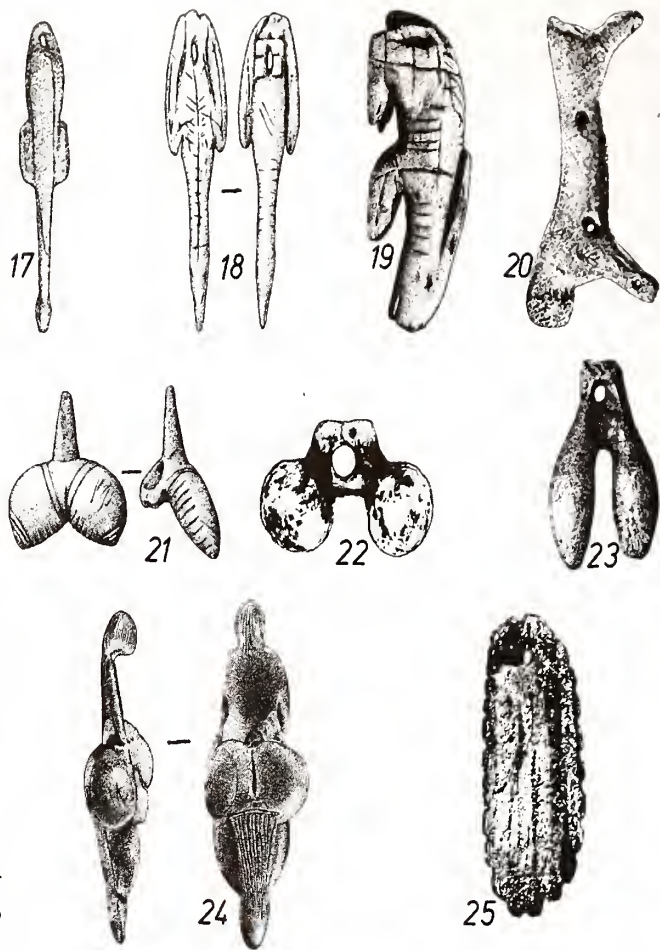
Among northern peoples, where skin garments predominate, seams became important parts of heraldic garment designs. Paleolithic carvers often rendered seams either as a single line with spurs (Type A):



or as two lines with interlocking spurs, sometimes called "toothed stitching" (Type B):



The custom of "dressing" tools and other objects is a common, widespread, ancient tribal pattern. For example, a seam of Type A is carved at the



6.17 through 6.25/ Variations of the inverted motif include bird (17, 18) and animal (19, 20) pendants, female figures (21-23), and other motifs (24, 25).

top of a Magdalenian ivory specimen from the Ukraine (fig. 6.26); its "garment" design is a basic genealogical pattern. An Okvik cup from the Bering Strait (fig. 6.27) "wears" a tailored garment stitched at the seams.

A seam of Type B, which is virtually a trademark of Thule art, occurs in many other cultures. We see it in a Paleolithic bone carving from southern Russia (fig. 6.28). An early needle case from the Bering Strait exhibits a combination of Type A and Type B seams (fig. 6.29). Both of these objects appear to be "dressed" in compartmented garments. The same motif, stacked solidly around a cylinder with a vertical divider, occurs on a number of ivory specimens from a Ukrainian site dated to 22,000 B.C. (fig. 6.30). Virtually identical specimens (four, to date) come from Canadian Pre-Dorset sites (figs. 6.31, 32). "Toothed-stitching" and vertical

dividers and the peripheral notching are evident on the specimen in Figure 6.32.

I see this design as an abbreviated genealogical pattern, with vertical lines separating opposing moieties. But no matter how one interprets the meaning, the form remains identical.

Drilled and Notched Ornamentation

Drilled ornamentation occurs in both European and Siberian Paleolithic art and was especially common in later Mesolithic art. Designs of dotted lines on Maglemosian pendants, as well as on Thule ear pendants and other ornaments, may represent seams. "Pockets" of this dotted motif are scattered along the Pacific Coast, over the Barren Grounds to the Maritimes, and across the Far North from Alaska to Greenland. Several of these pockets are fairly early, dating to Old Copper (3000 B.C.) or Laurentian (2500 B.C.) times. Thule examples are much later, of course, but earlier examples exist. I think the custom of marking seams with lines of drilled dots entered the New World both very early and very late. Figure 6.32 shows a peripherally notched bone cylinder from a Pre-Dorset context, dating to ca. 2200 B.C. In addition, there are peripherally notched bone beads from Moravia, dating to ca. 24,000 B.C. (fig. 6.33), and another from the Paleoindian Lindenmeier site in Colorado, dating to ca. 9000 B.C. (fig. 6.34). Other examples fill in the gaps between these.

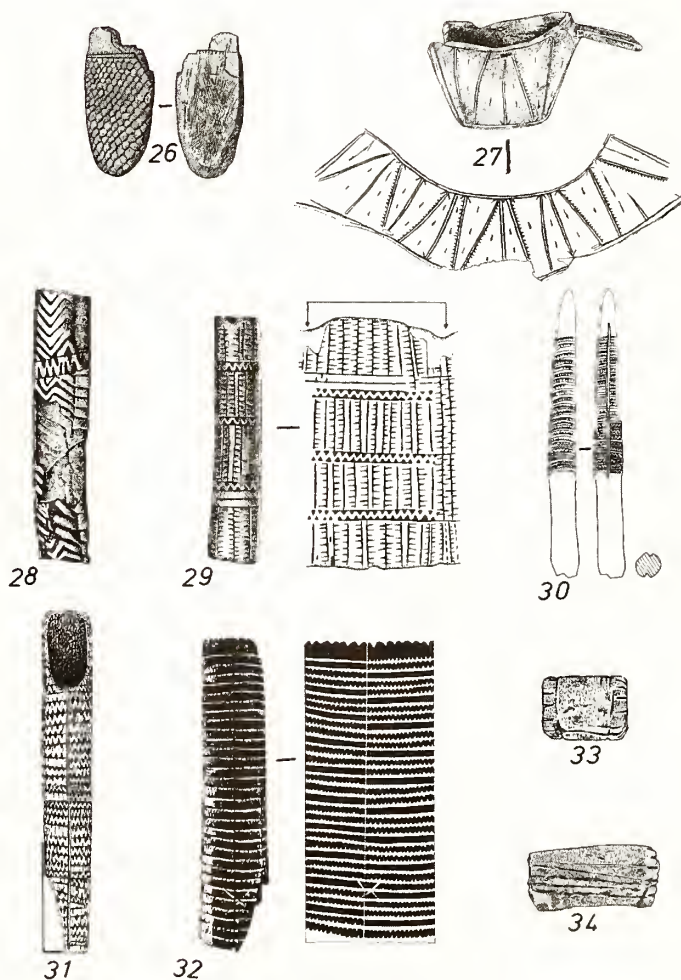
Tectiforms

"Tectiform" paintings on European cave walls look more anthropomorphic than architectural. For example, compare the paintings from a Dordogne cave (fig. 6.35) with two Siberian petroglyphs thought to be early (fig. 6.36). The form becomes more explicitly anthropomorphic on a Puvuk Eskimo comb from the Bering Strait (fig. 6.37). Then compare

the skeletal design on another Siberian petroglyph, also believed to be early (fig. 6.38), with a wooden effigy displaying classic Dorset engraving (fig. 6.39).

Other correspondences in art, such as semi-lunar notching, can be demonstrated, but these are mere details, perhaps accidentally shared. What is more basic is that Eskimo art resembles Paleolithic art generally: it has the same "feel." This is apparent to even the most casual observer. Obviously, the Dordogne was not the High Arctic. But Paleolithic and Eskimo hunters pursued a way of life that must have been, in many respects, fairly close. Smoldering embers from that ancient life survived in many parts of the world, perhaps most of all in the Arctic.

What survived primarily, of course, were not the outward forms of art but the underlying traditions that



6.26 through 6.34/ Seam motif (26–32) and drill and notched (33, 34) ornamentation

motivated that art. "Tradition" simply means "what is transmitted." What is transmitted is an attitude of mind, a shape of heart. The final artistic product, the object, is merely its afterlife. Its real life is how it got to be that way. Much of Eskimo art got to be the way it was through tradition, not through trade or invention.

Minute Carvings, Microscopic Engravings

One common attitude shared by Paleolithic and Eskimo artists was the challenge of minute workmanship. Aivilik Eskimos tell a story of a visiting Japanese artist who carved a face on the head of a pin and, then, his host carved a face on an eye of the first face. The story rings true, if only in principle. Eskimos would delight in that challenge. I saw a bear carving so small it passed through the sprocket-hole of a 35-millimeter film. I own a carving of a man with a child on his shoulders, so minute it requires optical magnification for identification. Modern souvenir carvers, equipped with dental drills and magnifying glasses, do not even approach the work of their ancestors.

Paleolithic carvers shared this ability. Perhaps they softened ivory in urine, the way Eskimos do, or wrapped the ivory in wet hides. That helps, a little. But mere technical assistance is not enough. What is required is skill, the kind that comes with commitment, rivalry, and, most important, a community of appreciative critics. Paleolithic and Mesolithic engravings are often so minute, so detailed, that we need microphotography to appreciate them. Hard to make, impractical to use: a personal challenge. Throughout history, few artists sought that challenge. Paleolithic and Eskimo artists did.

Living Art

Upper Paleolithic and Eskimo artists also both excelled at naturalism. Some Eskimo animal effigies are so realistic that we can distinguish between, for example, a red-throated loon and a common loon. This is equally true with much of Paleolithic art. Yet verisimilitude is rare, and especially rare in tribal art. It was far more common in later cultures, beginning with the city-states.

One explanation for optical realism is that possessing a likeness confers power over the original. Although this is an interesting idea that might have some truth in it, I know of no supporting evidence, save that Paleolithic images were sometimes used as targets.

Another theory holds that such images were not as much *lifelike* as they were *living*. Here, there is evidence. Eskimos preferred effigies that were made of organic materials: ivory, bone, wood. Eskimo carvers "released" the forms hidden within these once-living materials. Images were not lifelike in size, of course, but alive in spirit. Carvers whispered to the hidden forms, then greeted them as they emerged. Did Paleolithic carvers share this view? Certain carvings suggest they did. Clearly pre-existing forms had their say. "Found form," a concept basic to the Eskimo, may have been equally favored by Paleolithic peoples. Beneath the Eskimo concept lay the further notion that the carver's function was to release, or imbue, the spirit.

Eskimo effigies often have small inlays in their chests or throats. An ivory Dorset bear effigy from Alarnerk (fig. 6.40) has a hollow neck with a sliding lid. This closed cavity originally contained red ochre. Was that ochre designed to bring the bear to life? Inserting a "battery" inside a statue in order to animate it was a widespread custom in the tribal world. This custom survived among the ancient Greeks, who put *pharmaka* (magic-stuff) in hollow statues.

I think the red ochre in the Alarnerk polar bear carving was "magic stuff." When Australian Aborigines incised a stone *churinga*, an "ancestral image," and then dusted it with ochre, they said the ancestor "bled." What bleeds, lives. Paleolithic artists ochre-dusted images engraved on flat stones. It is possible that the ancient custom of dusting the dead with red ochre had the same purpose: to animate.

Two-Dimensional Art

Eskimos engraved minute, realistic silhouettes on bow-drills and other flat surfaces. For years, these were dismissed as Western-inspired. But then, prehistoric

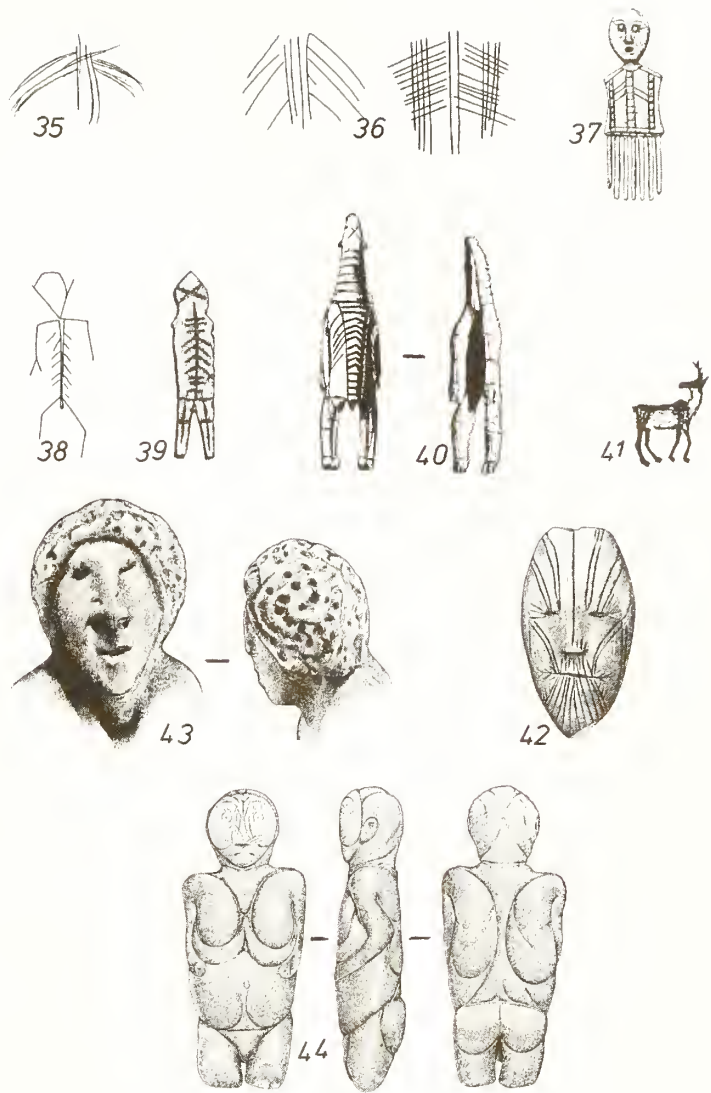
examples were recognized. Some showed complex “scenes” and many were “framed.” By contrast, Paleolithic art lacked borders. Conceivably, Eskimo borders derived from some later, probably Asian, tradition.

Borders aside, what is important here is the naturalism. A 1912 Eskimo pencil drawing of a caribou with its head turned back (fig. 6.41) could grace a cave wall in Paleolithic France. Both Paleolithic and Eskimo artists employed this silhouette technique. Both simulated depth by leaving a gap between an animal’s body and its far legs. And both sometimes depicted herds by using the “stutter” technique of parallel profiles.

Human Faces

Naturalism in Paleolithic art rarely extended to human images. Most heads were missing or crude or abstract, with no facial features. There is one rare exception (fig. 6.43) from the late Paleolithic site of Malta in central Siberia; in spite of its minute size, this head has a wonderfully naturalistic face, even to the addition of scalp holes presumably intended for hair inlays, much like later examples from the Bering Sea area. In other words, Paleolithic artists, when they chose to do so (which was not often), could render a good human likeness.

Exactly the same can be said of Eskimo artists. Faces were generally left blank or stylized with tattoos or even masked. Yet the earliest known human image from the Canadian Arctic (fig. 6.42) is near-portraiture. It comes from a Pre-Dorset Paleo Eskimo site (ca. 1900–1600 B.C.). Clearly, early Eskimo carvers, like their Paleolithic predecessors, could render a good human likeness when they chose to do so. Most chose not to do so. The Okvik artist who carved the image in Figure 6.44, although immensely skilled, hid the face behind a mask, which resembled leather examples worn by nineteenth-century Hudson Bay Eskimos.



6.35 through 6.44/ Tectiforms (35–39), living art (40), naturalism (41), and human face (42–44) motifs are all seen in Eskimo art.

Everyday Art

Several Canadian archaeologists have recently sought to explain why so many Dorset effigies lay abandoned in middens, unbroken. These were, we were told, amulets or phylacteries, discarded like broken watches or computers when their powers failed or faded (e.g., McGhee 1985). Since most of the surviving examples came from Terminal Dorset, we were further invited to believe that art proliferated at dusk—in a failed attempt to avert the demise of Dorset culture. In short, art was asked to justify its existence by performing some nonartistic service.

Amulets attached to garments, kayaks, and bags were often simply beast-parts, scraps of dead animals. Their magical powers did not depend on workmanship. Nothing religious required that amulets be exquisitely carved, just as nothing practical required that clothing be beautiful. Yet both were.

Many Dorset effigies show no means of attachment, no marks of use, and no obvious function. I suspect they had no function other than being. Eskimo art was an act, not an object, a verb not a noun. Carving was like singing: those who felt a song within, sang; those who sensed a form emerging from ivory, released it. Carvings were passed around, enjoyed, and discarded. Many lie in middens, undamaged and unused.

Critics who supply Eskimo art with excuses for being assure us that Dorset art was the handmaiden of shamanism. I doubt that. Shamanism was a specialized ritual, not to be confused with general belief. Shamans had professional gear, unique in form, and limited in number: drums, sucking-tubes, belts, fake teeth, coat-dangles, etc. This paraphernalia never constituted more than a tiny fraction of the total ensemble of Eskimo carvings.

Most Eskimo art belonged to the workaday. What I love most about it is its optimism. People who lavished care on a scraper or a wound-plug clearly regarded life as worth living. Common tools became works of uncommon beauty.

Visual Puns and Humor

Eskimo carvings often combined images of different creatures. These creatures should not mislead us. Not all of them depicted a World Apart. Puns, like nicknames, were a part of Eskimo daily life—the more bizarre, the better. When Eskimos chose to represent nature optically, they did so with great accuracy. On other occasions, they added lore or humor. Animals acquired anthropomorphic qualities; humans acquired zoomorphic qualities. Art became a playground for joyous pranksters.

Paleolithic artists enjoyed similar jokes. Remember those Magdalenian spear-throwers, each with a carved ibex whose head turned back to observe a bird sitting on a turd emerging from its anus? There is even one example of two birds “kissing” on such a turd. Eskimos would love that carving.

Discontinuous Continuity

For millennia, Eskimo artists absorbed ideas and styles from various sources. Fads came and went. Asian metal-age cultures introduced a death cult, mortuary art, professional carvers, joint-marks, animal *enroulée*, cheek-plugs, the nucleated circle with spur, and much more. Most of all, they introduced a metal-engraver’s flat art. Eskimos wrapped that art around three-dimensional ivory objects, tattoo fashion, to create Okvik and Old Bering Sea art.

That art delights us. But Paleolithic traditions proved tenacious. They did not come from outside but survived from within, transmitted by act and words from mother to daughter, father to son. The ideas behind them, even the forms those ideas took, survived intact until yesterday, and nowhere more strongly than in the Arctic.

Yet, vast gaps occur. Some arise from incomplete evidence. Other gaps may be real. The inverted-female pendant, first seen in the European Paleolithic and later in the Siberian Paleolithic, reappears in Late Eskimo times. “Toothed stitching” appears in Paleolithic Europe, resurfaces among Pre-Dorset peoples, and then fades from view until Thule times and later. Where were these motifs during these long intervals?

Ultimately, all of these forms came from Paleolithic Europe. More immediately, they came from northern Asia. From there, migrants took them east to Greenland and southeast to Oceania. Exactly when, we do not know. But whenever it was, those motifs by then were already ancient and had long ceased to be the exclusive property of any single tribe or culture.

It is ironic that the most recent newcomers into the Canadian Arctic, the Thule people, brought with

them some of the oldest traditions: “toothed stitching,” featureless faces, and inverted, steatopygous female pendants, among others. All of these motifs enjoyed a renewed popularity. Like smoldering embers, they burst back to life.

What goes out of sight need not go out of mind. Even when motifs faded from view, their mental underpinnings often remained in place. A symmetry of silent assumptions underlay each. “Toothed stitching” came from heraldic, mosaic garments. These continued to be made. Inverted effigies reflected a belief in an inverted After World. That belief remained widely popular among traditionalists. Surely, such underpinnings made it easier for these motifs to stay alive, even to reblossom here and there, like seeds in fertile soil.

In trying to explain this phenomenon, it helps to recall the central role of memory in tribal societies, especially in isolated Arctic societies. Life expectancy was short. Survival depended on knowledge. Knowledge and wisdom depended on elders, who were few in number. A group of McKenzie River Eskimos, in comparatively recent times, migrated to northern Greenland and lived there in isolation until 1818. They became known as the Polar Eskimos. There is no reason to believe they lacked bows or kayaks when they began their migration. But in 1818, although they vaguely remembered both, they possessed neither. Did tragedy mark that migration? How old were its survivors?

“Come, sit beside me. Do as I do,” says the seamstress to her little daughter. “Watch me,” says the hunter to his son. Living people are traditions’ safest repositories. They are the great preservers. And preserve, the Eskimos did, so successfully that we can trace a memory link through their art back to Paleolithic times.

Note

1. All but six of the specimens illustrated in this chapter, along with many other examples, are published in Carl Schuster’s and Edmund Carpenter’s

(1986-1988) multi-volume set, *Materials for the Study of Social Symbolism in Ancient and Tribal Art*, Rock Foundation, New York and were drawn by Kathleen Kitsos. The figures in this chapter can be found at:

(1) vol 3:3, p. 465; (2) vol 3:3, p. 468 (3) vol 3:3, p. 466; (4) vol 3:3, p. 466; (5) vol 3:3, p. 459; (6) vol 3:3, p. 458; (7) vol 3:3, p. 459; (8) vol 3:3, p. 462 (9) vol 3:3, p. 463; (10) vol 3:3, p. 475, (11) vol 3:3, p. 484; (12) vol 3:3, p. 465; (13) vol 3:3, p. 477; (14) vol 3:3, p. 480; (15) vol 3:3, p. 480; (16) vol 3:3, p. 482; (17) vol 3:3, p. 584; (18) vol 3:3, p. 585; (19) vol 3:3, p. 583; (20) vol 3:3, p. 583; (21) vol 3:3, p. 528; (22) vol 3:3, p. 607; (23) vol 3:3, p. 609; (24) vol 2:4, p. 901; (25) vol 1:2, p. 660; (26) vol 2:5, p. 1150; (27) vol 1:1, p. 322; (28) vol 2:3, p. 651; (29) vol 2:3, p. 651; (32) vol 3:3, p. 560; (33) vol 3:3, p. 558; (34) vol 3:3, p. 559; (35) vol 2:2, p. 506; (36) vol 2:5, p. 1268; (37) vol 2:5, p. 1268; (38) vol 2:5, p. 1268; (39) vol 2:5, p. 1268

The six exceptions are:

30: Engraved ivory specimen from Khotylevo 2, Briansk, Russia. After F.M. Zaverneyayav, “Une nouvelle station du Paléo-lithique superieur sur la Desna” (in Russian), *Sovietskaya Arkheologiya* 4, 1974, Moscow.

31: Engraved needlecase, Pre-Dorset site near Igloodik, Canada, circa 2000-1900B.C. Ht: 7.2 cm. After Jørgen Meldgaard, “Eet folk gennem 4500 ar?”, *Qeqertasussuk: De forste mennesker i Vestgronland*, *Qasigiannguit Museum*, 1900: 114-116.

41: Robert Flaherty, *Drawings by Enooesweetok of the Sikosilingmint Tribe, Fox Land, Baffin Island*, 1915, Toronto.

42: After James W. Helmer, “A Face from the Past: An early Pre-Dorset Ivory Maskette from Devon Island, N.W.T.,” *Etudes/Inuit/Studies*, 10, (1-2):179-202, fig. 3, 1986, Montreal.

43: Ivory figurine, Malta site, Siberia. After photograph by Alexander Marshack. See Henri Delporte, *L’image de la femme dans l’art préhistorique*, 1993, p. 199, fig. 24.

44: Okvik ivory figure, Punuk Islands, Alaska. Ht. 11 cm. Menil Collection, A8468, Houston.

Danish Arctic Archaeology From the Royal Society for Northern Antiquities to the Fifth Thule Expedition

JØRGEN MELDGAARD (PICTURED)
HANS CHRISTIAN GULLØV

In this chapter, we outline the beginnings of Danish Arctic archaeology as it was reflected in the initiatives of scientific committees of the nineteenth century, which were formed mainly to organize and collect information on Danish prehistory. We demonstrate how an understanding of the Danish past, in terms of the scientific description of the stratigraphy of peat bogs and human deposits, was expanded during now-forgotten comparative investigations of middens in Greenland. One result of those early excavations in the lowest parts of middens was the recognition that ancient Greenlanders had used stone tools exclusively. By the end of the century, however, this recognition had faded away. The scientific view of Greenland's past, in comparison to the Danish Mesolithic, was that Greenland was an ethnographic isolate, comparable to other remote areas of the globe.

When systematic Danish archaeology was initiated in the Arctic in the 1920s with the Fifth Thule Expedition, it relied heavily on a methodology steeped in the natural sciences. At the same time, it neglected the knowledge that had been gained in the previous century about the existence of a stone age culture in Arctic Greenland. Nevertheless, the results of that expedition were important to our understanding of the Arctic, and we end our chapter with a description of those Danish research efforts in Canada. During his archaeological excavations in Greenland between 1929 and 1934, while expanding his systematic work on the Thule culture, Therkel Mathiassen also "pushed the



Paleoeskimos out in the cold" and his processual reasoning about living and non-living resource exploitation (Mathiassen 1927a:158ff) became a cue for later Pre-Thule research.

Infancy of Arctic Archaeology

In 1824, a small collection of artifacts from Greenland arrived at the Museum of Northern Antiquities in Copenhagen. It included a small runic stone from the Upernavik district and other objects, some of which were of stone (Rosenkrantz 1967). The collection was incorporated into the Museum, which was then under the directorship of C. J. Thomsen (1788-1865), the "father" of the three-period system (Stone, Bronze, and Iron ages). In 1816, Thomsen had been appointed Secretary of the Antiquities Commission, which had been responsible for the Museum (later the National Museum) since its foundation in 1807.

A description of the runic stone was published in 1827 (Rosenkrantz 1967) and Thomsen, as a member of the Royal Society for Northern Antiquities, which had been established in 1825, inspired the Society's

members to extend their interest to Eskimo prehistory. Between 1832 and 1841, the Society sent out letters urging government officials in Greenland to undertake excursions and searches for antiquities. Their first priority was the collection of Norse antiquities. At their meetings, the Society's members discussed artifacts from Nordic countries and North America, publishing their accounts in the Society's journal, *Annals of Northern Antiquity*, which had started in 1831. In the second volume (1833), the journal recounts the first meeting at which old "Eskimo artifacts" were presented.

A report on the first large collection of Eskimo artifacts, however, was presented to the Society in 1838. The collection consisted of stone objects, all of which had been found in abandoned houses a quarter of a mile from Jakobshavn, i.e., *Saqqaaq* artifacts from Sermermiut in Disko Bay. Another presentation on a similar collection was made in 1843, when the Society's secretary, Christian Pingel, reported on stone artifacts. A geologist, Pingel had a special interest in raw materials and techniques, and he went into detail in his report with examples of "arrowpoints of dark-green chalcedony, finely indented on both edges . . . [and] utensils of *ângmâq*, the collective term used by the Greenlanders for various stone materials, all of which seem to originate in little-known formations of the clay-slate layers in North West Greenland" (Meldgaard 1996).

That same year, 1843, the Society also established the Museum Americanum, Cabinet of American Antiquities, which was designed to house artifacts from the North American mainland. After announcing this initiative to its members in America, the Society began to receive more packages of artifacts during the next few years. The invitation to American members presumably had been inspired by the arrival of a particularly interesting package from Boston with an artifact that had been discovered some years earlier in 1831. This package contained fragments of a coat of mail found in the grave of a man of great height and—according to medical evidence—with a "non-Indian"

skull type. The find, which came from near Fall River in Massachusetts, only 30 km north of the round Newport Tower, was interpreted as a significant Scandinavian relic from the Norse "Vinland." Henry Wadsworth Longfellow, then a young literary historian from Harvard touring Europe in 1831–1832, met with the Society's secretary, C. C. Raffn, in Copenhagen. Raffn asked him about the possibility of Viking remains in New England. They kept in touch, and later Longfellow learned that an analysis of the "bronze-armor" found in the Fall River grave showed that the skeleton was a Scandinavian Viking! Longfellow related the story of the Viking's voyage across the ocean to Vinland and then to his Fall River grave in his poem "The Skeleton in Armour" (Meldgaard 1993).

The "skeleton in armour" specimen and other artifacts from the Museum Americanum were later transferred to the Museum of Northern Antiquities. Beginning in 1855, they were housed in the building where the Royal Ethnographic Museum had opened its collections to the public in 1849. From 1866, when J. J. A. Worsaae (1821–85) succeeded Thomsen as director of the Museum of Northern Antiquities and the Ethnographic Museum, to the present, the find has been stored on a shelf in the Amerindian section of the Ethnographic Museum—now as the remains of an Indian warrior. Worsaae arranged the archaeological collections of the Ethnographic Museum as a counterpart to the Museum of Northern Antiquities, and the rest of the collections as comparative material for archaeology. The collection of ethnographic objects almost ceased, and Thomsen's international contacts were broken off (Lundbæk 1988:175).

As interest in curiosities declined, Danish archaeologists, led by Worsaae, took a more scientific approach, studying the relationships between different sites and stratigraphic units and the artifacts found in them. The initiative for this came from a newly established committee called the Kitchen Midden Committee, which introduced the method of stratigraphic investigations into Greenlandic archaeology.

The Kitchen Midden Committee

In the 1830s, the zoologist Japetus Steenstrup's (1813-97) fundamental stratigraphic studies of Danish peat bogs demonstrated, through the analysis of faunal and floral remains from various layers, that nature had undergone a process of evolution. This work showed that the peat bogs were veritable "archives" of Danish prehistory, and it became the basis of Danish peat bog research, which subsequently grew into a fruitful collaboration with the fields of geology, botany, zoology, and archaeology. In 1848, Steenstrup initiated the so-called Kitchen Midden Committee, whose three members—geologist Professor Forchhammer, archaeologist Worsaae, and Steenstrup—were appointed by the Royal Danish Academy of Sciences and Letters. The committee's task was to determine whether the shell heaps that contained scattered occurrences of animal bones and flint tools along the Danish coasts were natural deposits, i.e., raised layers of shells, or whether they were the results of human activity. The committee's excavations confirmed that they were culturally formed, resulting in a great step forward in archaeological and Quaternary-zoological research (Aaris-Sørensen 1988:18ff).

In the 1860s, Steenstrup and Worsaae disagreed over the formation and period of the peat bog middens. Unlike Worsaae, Steenstrup supported the theory that the middens were a universal cultural phenomenon and believed that this idea could be backed up by evidence from middens in Greenland. Requests for information were sent to colonists in Greenland, and in the 1870s reports from Disko Bay and Nuuk/Godthåb began to reach the Zoological Museum.

In 1871, Carl Fleisher, the local factor in Claushavn and an uncle of Knud Rasmussen, carried out excavations at the almost-inaccessible site of Qajaa in Jakobs-havn Ice Fjord and carefully described the midden stratigraphy. He sent Steenstrup boxes of artifacts and animal bones and a report that began:

It is a bad thing up here in Greenland that the soil is frozen all summer when you dig down

... but I do hope this stuff will be of some use to you. I have made every effort, but it is a difficult thing to collect bones when you do not know if they are of importance.

Fleisher's report featured an astonishing and detailed description of the stratigraphy, starting with the bottom layers of bones and stone artifacts, then by a sterile layer of peat, and followed by an upper midden that contained no stone tools. His analysis of the bone material from various layers indicated that during certain periods the Ice Fjord had been "less filled up with ice" and that "the bottom layers represent a period when the old Greenlanders used stone only," indicating that there had been a stone age culture in Greenland before the beginning of the use of iron. Steenstrup made good use of the Greenland material, discussing it in lectures on the Younger and Older Kitchen Middens in Greenland he gave in Denmark and abroad in 1872, but he failed to mention Carl Fleisher and the Greenlanders who provided him with the material and with their thought-provoking comments (Meldgaard 1996).

Another contributor to the Greenland midden debate was Lars Møller, a well-known printer and editor in Godthåb. The material and report he sent to Steenstrup are in the Zoological Museum archives, where they were found in 1986 by Morten Meldgaard. Møller's report describes his 1874 excavations at Kangeq and Illorpaat on Hope Island; it also indicates that he was shipping (in 1875) boxes of "samples of the upper and lower layers of the middens" to Copenhagen. His report contains a great amount of detail about the special way he handled and packed the samples from the 4-foot-deep midden at Illorpaat; he writes that, "in the box you will find it just as it was when taken from the soil, i.e. with the uppermost layers in the upper part of the box and the lowermost beneath." He followed the same procedure with the 8-foot-deep midden at Kangeq.

Although Fleisher's and Møller's research was subsequently forgotten, and their reports were hidden away in archives for more than a hundred years, Steenstrup was able to prove his theory that the middens were, in

fact, a universal phenomenon and not restricted to certain periods and peoples. The results of stratigraphic excavations did not lead Danish archaeologists to speculate any further about the time-depth of Greenlandic middens, although the investigations, in fact, had been undertaken to test a phenomenon in Danish prehistory and the stone artifacts described were similar to those of the Danish Mesolithic. Not until the 1970s and 80s could archaeologists confirm the observations and conclusions of these early investigators in Greenland (Gulløv and Kapel 1979–80, 1988; Meldgaard 1983, 1991; Møhl 1986).

Among the officials who continued to collect artifacts in Greenland, encouraged by the initiatives of the Royal Society for Northern Antiquities, was C. G. F. Pfaff, a physician in Jakobshavn from 1854 to 1876. His interest in prehistory stemmed from his purchase, from local Greenlanders in the Disko Bay area, of a large collection of artifacts taken from graves and excavations. Pfaff systematically arranged the artifacts on boards according to their function and material. When he retired and returned to Denmark, he tried to donate his collection to the National Museum, but the director, Sophus Müller, was not interested in it, seeing it only as comparative material for the Danish collection and noting that “we already have a lot of this stuff” (Westman and Jakobsson 1989). Eventually, the Ethnographic Museum in Stockholm acquired Pfaff’s important collection, and today it is housed in the Center for Arctic Cultural Research at Umeå University (Westman and Jakobsson 1989).

Parts of the Pfaff collection were described in 1904 (Svenander 1906) and again in 1907 by the Norwegian historian and geographer Ole Solberg (1907), whose study of the stone age in Greenland was inspired by a newly discovered Norse source, *Historia Norvegiae*, which alluded to conflicts between the early Norse settlers and stone age peoples. In his work, Solberg mentioned the Pfaff collection in Stockholm along with his studies of stone artifact collections in Oslo and Copenhagen. Solberg’s (1907) conclusion—

that a stone age culture had existed in West Greenland, with a core area around Disko Bay, some of it extending far into the past, i.e., to before A.D. 1000—however, had little impact on Danish scholars.

A New Era

In 1878, a Commission for Scientific Research was established to conduct geographical and geological explorations in Greenland. Its expeditions were often led by naval officers, who were also given charge of the archaeology. From 1884, when Europeans first met the East Greenlanders in Ammassalik, to 1912, when Ejnar Mikkelsen returned from his Alabama Expedition, the Commission’s first priority was the exploration of East Greenland.

The world in general was fascinated by the fact that an Eskimo population, which had been encountered by Captain Clavering in 1823, still existed north of Ammassalik. During the first expeditions to this region, native Greenlanders were assigned to the crew in the event that it should encounter new groups. These expeditions—the Scoresby Sound Expedition in 1891–1892, the East Greenland Expedition to the Ammassalik district and the Blosseville coast in 1898–1900, and the Denmark Expedition to North East Greenland in 1906–1908—produced the first scientifically described archaeological collections from house ruins and graves (Ryder 1895; Thalbitzer 1909; Thomsen 1917; Thostrup 1911).

Researchers studying the collections from these expeditions concluded that an Eskimo culture, which was comparable to the one demonstrated by similar finds from Canada and Alaska, existed in Greenland. In addition, discussions on Eskimo migration routes inside and outside Greenland emerged, focusing on the origins of Eskimo culture, as the geographer H. P. Steensby (1875–1920) had postulated on the basis of deductions from ethnographic data (the so-called anthropogeographical method).

Although more than twenty years passed before Greenland was again investigated archaeologically,



7.1/ *Knud Rasmussen, nephew of Carl Fleisher ("my learned uncle Carl"), and leader of the Fifth Thule Expedition*

major inroads were made on the ethnography of the Eskimo culture with the work of Knud Rasmussen (fig. 7.1). In 1909, Rasmussen outlined an ambitious research plan, entitled "Proposal for a Danish Ethnographic Expedition to the Central Eskimos," which was designed to investigate the homogeneity and origins of the Eskimo culture. Four other expeditions, partly financed by the fur trade income of the Thule Station, however, were planned and completed before Rasmussen could launch the Fifth Thule Expedition in 1921 (Mathiassen 1945).

Fifth Thule Expedition to the Central Eskimos, 1921–1924

Seventy years have passed since the last members of the Danish expedition to the North American Arctic returned to Denmark. For scientists the world over who use the inexhaustible material collected during the expedition, it still stands as a milestone in Arctic research, but the circumstances under which the expedition operated have mostly been forgotten.

We would like to describe the efforts of those few people who brought new knowledge to the sciences and humanities, using such simple means as notebooks, spades, tape measures, cameras—and dog sledges. Although they have all passed away, born as they were

in the nineteenth century, their pioneering work in the fields of archaeology, anthropology, and ethnography had far-reaching consequences for the understanding of the Arctic and its peoples (de Laguna 1979).

From the beginning, the expedition was planned as a continuation of Rasmussen's work in Greenland, where he had collected myths and tales and studied customs and usage. New questions were added: the origins of the Greenlanders, and the development and adaptation of Eskimo culture in the enormous and varied geographical regions between

Canada and Alaska. To address these questions, Rasmussen had to combine research in archaeology and ethnography with the study of geography and natural history.

Despite the large scale of research, Rasmussen (born 1879) had to limit the number of participants ensuring that each member, in addition to general talents, had a broad range of specializations. His friend and companion of many years, Peter Freuchen (born 1886) was a natural choice. He had fifteen years of experience in arctic research and had long been in charge of the Thule Station.

Therkel Mathiassen (born 1892) was chosen as archaeologist and cartographer. Rasmussen found him far from Copenhagen, in Jutland, where he worked as a secondary school teacher. Mathiassen's research on the Danish Mesolithic peat bog culture, based on excavations with his friend, the geologist Lauge Koch, had been refused publication by the Royal Society of Northern Antiquities. His application for a post in the Department of Danish Prehistory at the National Museum had been rejected by the director Sophus Müller, who claimed that he could see no connection between Mathiassen's university degree in geography and natural history and a position as museum curator. Thus, Mathiassen, who had recently been married, was forced



7.2/ Map of the Fifth Thule Expedition

to find another occupation. It was to Rasmussen's great credit that he could see beyond the qualities of an academic or professional "nobody." His choice of Mathiassen as the expedition's archaeologist also meant that the methods developed by Steenstrup and Worsaae would be reintroduced into arctic research.

Kaj Birket-Smith (born 1893) was selected as ethnographer and geographer. He had completed two expeditions to Greenland and had finished a comprehensive comparative study of the Greenland collections at the National Museum's Department of Ethnography with material from his 1918 fieldwork in the Egedesminde district (Birket-Smith 1924). Trained in the same subjects as Mathiassen and employed by the recently independent (since 1920) department, he represented the National Museum on the expedition. Several other Danish members also participated, including Rasmussen's assistant Helge Bangsted and, from 1923 onwards, cameraman Leo Hansen who joined him on the sledge journey from Coronation Gulf to the Bering Strait.

Seven Greenlanders also participated in the expedition. They were the West Greenlander Jacob Olsen, who served as interpreter and secretary (valuable in both archaeology and ethnography), and the Polar Eskimos Arqioq and his wife Arnánguaq; Nasaitdlorsuarssuk and his wife Aqatsaq; the widow Arnarlúnguaq; and the young hunter Qavigârssuaq.

In 1921, the expedition's Danish members left Copenhagen on board the steamer *Bele* in the company of a select group of ecclesiastical authorities bound for Godthåb to attend the celebrations honoring the arrival of the missionary Hans Egede in 1721. Several weeks earlier the expedition's schooner *Søkongen* had left Denmark. A third ship, which also became important for the course of the expedition (fig. 7.2), was the steamer *Iceland*, also bound for the festivities in Godthåb with the Danish King and the Royal Family on board.

From Godthåb, the *Bele* sailed northward along the west coast of Greenland with, among others, Birket-Smith and Mathiassen. One foggy morning between Uummannaq and Upernavik, the ship met her fate on a

submerged reef. The passengers and crew escaped to a little desert island but had to leave the equipment on board. The King's ship and *Søkongen* saved the shipwrecked travelers, but all of the expedition's equipment had been lost. Over the *Iceland's* telegraph, Rasmussen asked for replacements to be sent to Godthåb with the first ship.

Rasmussen refused to give up his plans, and the *Søkongen* continued northward to Thule, where the Polar Eskimos came aboard with seventy sledge dogs. As they sailed back to Godthåb, new misfortunes struck. Peter Freuchen's wife, Navarana, died of pneumonia in Upernavik, and later one of the Polar Eskimos succumbed to the Spanish flu.

In late September, five months after leaving Copenhagen, the expedition finally reached its field of work in spite of engine failures and heavy pack ice in the Hudson Strait. On a small island called Danish Island, it built an expedition house from materials originally meant for the wooden provisions shed. "The Belows" was to be home base for the next two years.

In December, expedition members met Eskimos for the first time. Rasmussen recounted this event:

Three or four miles ahead a line of black objects stood out against the ice of the fjord. I got out my glass; it might, after all, be only a reef of rock. But the glass showed plainly a whole line of sledges with their teams, halted to watch the traveller approaching from the South. One man detached himself from the party and came running across the ice in a direction that would bring him athwart my course. . . Without waiting for my companions to come up, I sprang to the sledge, and urged on the dogs, pointing out the runner as one would a quarry in the chase . . . Stand still! I cried; and, taking a flying leap out among the dogs, embraced the stranger after the Eskimo fashion. . . I had yelled at the dogs in the language of the Greenland Eskimo. And, from the expression of the stranger's face, in a flash I realized that he had understood what I said. He was a tall, well-built fellow, with face and hair covered with rime, and large, gleaming white teeth showing, as he stood smiling and gasping, still breathless with exertion and excitement. It had all come about in a moment,—and here we were! (Rasmussen 1927:3–4).

The scientific work got started. At first, expedition members made short sledge trips from Danish Island to their closest neighbors, a group of about 100 Eskimos who moved along a 500-kilometer stretch of the coast. Then, coordinated by Rasmussen, the members branched out. The most promising fields for archaeological work were expected to be found north along the Melville Peninsula and on northern Baffin Island, the least-known stretches of the coast. Of greatest importance to the ethnographic work were the inland Eskimos of the Barren Grounds to the southwest. Rasmussen worked among these peoples in 1922 and Birket-Smith during two periods in 1922 and 1923.

In January and February 1922, Birket-Smith and Jacob Olsen sledged southward to Chesterfield Inlet and Baker Lake, followed by Rasmussen and Bangsted in March. Along the Kazan River they met the Harvaqtormiut, "the people of the river whirlpools," of whose existence they had been unaware. Although the native men had encountered white men during journeys to the newly established Hudson's Bay Company trading post at Baker Lake, the women and children found the white men new and mysterious creatures. The expedition team spent the summer among the largest of these groups, the Paallimiut, "the people of the willows" near Hikoligjuaq Lake, documenting their material culture and recording their myths, poems, and tales. Little by little, Birket-Smith gained an understanding of the exceptional status of these inland Eskimos compared with other known Eskimo groups. Named generically Caribou Eskimos, they became the central focus of ethnographers and Eskimologists during the next few decades in discussions of the origin and evolution of Eskimo culture. Birket-Smith believed that these Eskimos, who had no knowledge of the sea or of the hunting of sea mammals but relied instead entirely on caribou for their nutrition, were survivors of an old Proto-Eskimo stage (Birket-Smith 1929, 5:232). The results of his studies were published with Rasmussen in three volumes (Birket-Smith 1929; K. Rasmussen 1930a, 1930b).

At the same time, in February 1922, Mathiassen, Freuchen, Arqioq, and Arnanguaq traveled northward by sledge. While snow covered the ground, their main tasks were cartography, geology, and ethnography. For the first 500 kilometers, they followed a route directly to Iglulik, encountering bad weather, with snow blowing from the north and temperatures hovering between minus 40 and 50 degrees Celsius. The dogs were in poor condition, and their food had to be rationed. Under these conditions, Mathiassen preferred to walk, setting out in the early morning before camp had been dismantled, being overtaken by the sleds around noon, and reaching the next camp after the snow house had been built. He described his routine matter-of-factly: "this gave me an opportunity to make detours and investigate geological formations, Eskimo ruins, etc., and to do collections of different kinds. . ."

Thirty-two years after Mathiassen's journey, one of the authors (Meldgaard), with Mathiassen's report in hand, followed in his footsteps, walking along a limited part of the route during the relatively pleasant summer months. It was absolutely amazing to note the amount of reliable information that he had collected during his walk in the snow and the cold.

The Eskimos, too, were impressed and a little awed by Mathiassen. Thirty-two years later, the older ones remembered him as the tall man who asked about remains of the old Tunit people. They had thought he was one of their descendants, and perhaps an avenger. Like the Tunit, Mathiassen traveled without dogs and was tall and strong. Freuchen, too, was by no means undersized. The two of them made a profound impression on the Eskimos—and created new myths.

Mathiassen reached Iglulik in April. The expedition stayed there for a few days, long enough for Mathiassen to collect some objects and write in his diary. These diary notes later formed the main source of information for his monograph on the material culture of the Iglulik Eskimos (Mathiassen 1928). After leaving Iglulik, Mathiassen and Freuchen went separate ways to map and study the geography of northern Baffin Island.

Freuchen traveled northwest along the coast until he was forced to return to Iglulik, while Mathiassen crossed the island to Admiralty Inlet before returning to meet Freuchen. Together, they mapped several hundred kilometers of the coastline and assigned Danish place-names to numerous islands, forelands, and lakes. They reached Danish Island during a blizzard at the end of May.

Mathiassen began archaeological excavations that summer. The most extensive took place at the site of Naujan in Repulse Bay over the course of two months. He excavated twelve old house ruins and recovered about 3,000 artifacts, to which he devoted the bulk of his voluminous treatise on the archaeology of the Central Eskimos (Mathiassen 1927b). Mathiassen concluded that the site had been inhabited by a thousand-year-old culture that had subsisted on whaling. He introduced the appellation "Thule culture" since finds belonging to a similar culture had been found at Thule in Greenland during Rasmussen's Second Thule Expedition. In terms of the origin of the Eskimo culture as a whole, he concluded that the Thule culture had migrated from Alaska to Greenland. His conclusions differed from those of Birket-Smith who, on the basis of ethnographic information, contended that the Thule culture had originated from Protoeskimo culture in the Canadian tundra. A fruitful discussion on the subject ensued (cf. Birket-Smith 1930; Mathiassen 1930b).

After finishing their excavations at Naujan in August 1922, Mathiassen and Olsen traveled to Southampton Island to continue their archaeological investigations. Although they had planned to stay only for a fortnight, because of bad ice conditions in Frozen Strait they were forced to remain on the island for six months. They lived among a small group of Eskimos, who regarded the uninvited guests with mixed feelings. When the Eskimos were afflicted by a serious influenza epidemic that winter, they attributed it to Mathiassen's investigations of the graves of their forefathers. During a sledge journey that winter, bad weather forced Freuchen to stay overnight in the open air; he got

frostbite in his foot, loosing a heel and some toes. As a result of this setback, they had to change their plans. Freuchen and Bangsted remained on Danish Island for another winter while Mathiassen traveled north, this time going all the way to Pond Inlet, where he completed his excavations in Canada and returned to Denmark in the autumn of 1923. Birket-Smith traveled southward to continue his studies among the Caribou Eskimos and then returned to Europe.

In March 1923, Rasmussen left Danish Island to begin his long sledge journey westwards, accompanied by the Polar Eskimos Qavigârssuaq and Arnarlúnguaq. He completed his trip the following September. At the end of his journey, standing on the Asian side of Bering Strait, he recalled the highlights of the past years:

The height on which I stand, and the pure air which surrounds me, give me a wide outlook, and I see our sledge tracks in the white snow out over the edge of the earth's circumference, through the uttermost lands of men to the North. I see, as in a mirage, the thousand little native villages which gave substance to the journey. And I am filled with great joy; we have met the great adventure which always awaits him who knows to grasp it, and that adventure was made up of all our manifold experiences among the most remarkable people in the world! Slowly we have worked our way forward by unbeaten tracks, and everywhere we have increased our knowledge. How long have those sledge journeys been?—counting our road straight ahead together with the side excursions up inland and out over frozen seas, now hunting game, and now seeking out some isolated and remote people? Say, 20,000 miles; more or less,—nearly the circumference of the earth. Yet how little that matters, for it was not the distances that meant anything to us. (Rasmussen 1927: iv-v)

Although many scholars in recent years have carried out more scientifically sophisticated research, no one has ever approached the scope of Rasmussen's accomplishments in terms of the collection of basic ethnographic data on Eskimo groups (cf. Kleivan and Burch 1988). His contribution to a better understanding of these peoples is a major achievement because of his

profound insights into the Eskimo language and an intuitive feeling for social demeanor. We do not agree with Remie (1988), who claims that Rasmussen's results were superficial because of the extensive range of his ethnographical work.

The expedition brought back more than 20,000 items, which were registered and distributed to Danish museums. The largest part of the collection consisted of 3,100 ethnographic and 11,100 archaeological artifacts, along with numerous geological, zoological, and botanical specimens. These objects were itemized in the series *Report of the Fifth Thule Expedition, 1921-24*, vols. 1-10, 1927-52, in 32 papers consisting of more than 5,500 pages. A fifth of this material is based on Mathiassen's archaeological, ethnographic, and geographical work.

One Hundred Years in Retrospect

During the hundred years between 1824 and 1924, Danish archaeological research in the Arctic developed into an independent discipline—Eskimo archaeology—separate from Norse archaeology. Systematic archaeological investigations of the Eskimo and Norse cultures were initiated in 1921 with the Fifth Thule Expedition and the archaeological and historical study of the buried Norsemen at Herjolfsnes (Nørlund 1924).

The Fifth Thule Expedition's main archaeological objectives were to investigate the origin and expansion of the Eskimo culture. Mathiassen resolved the issue of origins by defining and describing the Thule culture. He did not, however, find any evidence of Solberg's stone age culture or of Steensby's Paleo-eskimo period in any of his investigations. He wrote that neither one had "appeared at any of the excavations at a total of ten places in the central regions; everywhere we find at the bottom of the refuse heaps and in the earliest ruins a typical Thule culture, bearing in fact a stronger stamp of marine animal hunting the deeper we go" (Mathiassen 1927a:200).

For Mathiassen, the "remains of the stone age people" found in Greenland in the 1870s simply did

not exist. For years, he refused to recognize what had been pointed out to him as pre-Thule artifacts in his own collections (de Laguna 1979). He was convinced that the burins were, in fact, boot-creasers splintered by use. When he saw the first manuscript describing the Greenland Saqqaq collection in 1950, written by one of the authors (Meldgaard), Mathiassen said, referring to the descriptions of burin types, "You'd better return to European archaeology. Burins were not used in Greenland—or in North America at all!" (Meldgaard 1952).

Despite some of the failings of his work, Mathiassen displayed great foresight in cooperating with local populations to conduct his research. To a great extent, he used the knowledge of Canadian Inuit and the Greenlanders to identify the function of artifacts and of the meaning of animal bones that he recovered from excavations. He also carefully instructed his native assistants in the method of stratigraphic excavation.

Mathiassen's contributions extend to his early studies of the present-day use and significance of artifacts, dwellings, and structures in the living community (e.g., Mathiassen 1928) and of how these material objects could have been incorporated into the archaeological record (Mathiassen 1927b). He used these observations as an indirect approach toward understanding the community of the past, and as a result, successfully contributed to the field of ethnoarchaeology.

When he continued his archaeological investigations in Greenland at the request of the Commission for Scientific Research, he used his previous experience to guide the scientific work of his assistants. These individuals became prominent researchers in Arctic prehistory in their own right. They discovered new cultural horizons in southern Alaska (Frederica de Laguna, his assistant in 1929); described new cultures in northeastern Greenland and Ipiutak (Helge Larsen,

his assistant in 1930); and extended our knowledge of prehistoric Greenland by defining the late Dorset and Ruin Islanders in Thule culture (Erik Holtved, his assistant in 1933 and 1934). In turn, *their* assistants—Eigil Knuth (Larsen's assistant in 1935) and Jørgen Meldgaard (Knuth's assistant in 1948 and Larsen's in 1950)—continued the work in the Eastern Arctic. They retrieved the Paleoeskimo culture from archives and field, and defined the Independence, Saqqaq, and Dorset cultures.

Our understanding of the prehistory of the Eastern Arctic has grown enormously since 1824, when the small collection of artifacts arrived at the Museum of Northern Antiquities in Copenhagen. Cultural periods covering a thousand of years of human activity have been described. Cultural meetings have taken place. New social structures and cultural material types have been identified. Danish archaeology systematics and Thomsen's and Worsaae's work on relative dating and typology have become guidelines for European archaeology (Renfrew and Bahn 1991:23, 98) and for subsequent fieldwork in the Arctic initiated by Mathiassen and continued by his successors.

We are indebted to our elders in the field and to our Inuit participants and friends, who in recent years have begun reaping the benefits of archaeology. Mathiassen and his assistants brought some 100,000 artifacts from Greenland to the National Museum in Copenhagen. This material has now been divided between the national museums in Copenhagen and in Nuuk, following the 1984 Agreement on the Transfer of Cultural Objects from Denmark to Greenland (Berglund 1994; Schultz-Lorentzen 1987, 1988). Nunatta Katersugaasivia Allagaateqarfialu—the Greenland National Museum and Archives—has itself taken over the tasks that previously were the responsibility of the National Museum of Denmark.

Analogy in the Ethnohistory of Greenland:

Learning from the Elders

HANS CHRISTIAN GULLØV

The use of analogy in archaeology has long served to help construct views of past lifeways (Stahl 1993:235). The method remains popular despite criticism that analogical reasoning restricts archaeological interpretation to the existing range of ethnographically observed behaviors (Wobst 1978). In the Arctic, the extreme environmental challenges faced by native peoples has been a major basis for justifying the use of analogy in archaeological reconstructions of the past.

In the Eastern Arctic, our knowledge of Eskimo cultures of the last millennium relies heavily on the pioneering work of Therkel Mathiassen and his experience as a member of the Fifth Thule Expedition from 1921 to 1924. Mathiassen demonstrated that very similar archaeological assemblages were recovered at most prehistoric sites throughout the Eastern Arctic. Although Mathiassen recognized that these assemblages reflected some differences that were attributable to geographical and temporal distances, as well as to subsistence activities, he argued that the similarities between assemblages were much stronger than their differences. His intersite comparisons left no doubt about the continuity of the prehistoric cultural tradition and by drawing on analogy with contemporary Eastern Arctic native culture, Mathiassen felt justified classifying all of these archaeological expressions in the Eastern Arctic under the appellation of Thule culture (Mathiassen 1927a:3).

Mathiassen's ethnographic fieldwork exposed him to the external constraints (ecology, technology, economy) operating on Inuit culture. When he returned



for his later archaeological research in Greenland, he brought this perspective with him. In later years, Mathiassen relied heavily on his experiences in the Canadian Arctic to derive his interpretations of Greenland prehistory, arguing that analogies based on Central Canadian Inuit culture were more accurate than those derived from Greenland, where more than two centuries of Danish colonial rule had introduced novel elements. As a result, our perceptions of Greenland Neoeskimo cultures are strongly influenced by Mathiassen's observations of Central Canadian Inuit, and these influences still pervade contemporary research in Greenland (Gulløv 1992).

Perceptions of the past can also be derived from oral traditions. Mathiassen (1927a:190) was aware of the Tunit legends, stories the Inuit told about an ancient race of dim-witted giant people who had been defeated by Inuit ancestors, and he presumed these stories had a historical background connected with the disappearance of the ancient Thule culture. Rethinking the Tunit legends three decades later, Jørgen Meldgaard drew attention to the concordance between

the Tunit oral traditions and his archaeological research at Dorset sites in the Igloodik region of the Canadian Arctic. Meldgaard believed the oral traditions confirmed his archaeological evidence of the succession of Dorset Paleoeskimo and Thule Neoeskimo cultures. He also believed that "the stories give a much more vivid picture of the Dorset people than we could have obtained from the archaeological sources alone" (Meldgaard 1955:172).

While recognition of a later Thule culture preceded by a Paleoeskimo sequence culminating in Late Dorset is now universally recognized by Eastern Arctic archaeologists, the nature of this cultural succession is not entirely clear from the archaeological record. Archaeological interpretations rely heavily on radiocarbon dating, which has a long problematic history in the Arctic (Arundale 1981; Maxwell 1985:253; McGhee and Tuck 1976), and archaeological research to date has not sufficiently explained the late- and terminal-period Dorset artifacts that appear in Early Thule, nor how and why Dorset traits reappear in certain areas in the Eastern Arctic (Gulløv 1996; Park 1993). Neither do the Tunit legends provide unequivocal proof that contact between Dorset and Thule peoples ever occurred. It has been argued that Thule interest in abandoned Tunit houses and artifacts may actually have produced the stories told centuries later to Meldgaard (Park 1993:220). In this chapter, I explore the possibility of reconciling contradictions between archaeology and oral history.

The Principles of Connections

Analogy is an integral feature of ethnohistory. In confirming the connection between a given prehistoric context and its historic counterpart, a continuity of intervening events can be observed. In this way the use of analogy in ethnohistory is different from that in archaeology, which has to rely on logic to demonstrate similarities between ethnographic sources and archaeological materials (Wylie 1985:95). Archaeological reasoning concerning the use of analogy recalls

the idea that cause should be found through comparison and inference, "for the same effects have the same cause," an idea put forward a hundred years ago by the former director of the Danish National Museum (Müller 1897:695; Ravn 1993:62).

In cultural historical research today, the claim of causality has epistemological implications and depends on the interpretation of the records and sources, which always takes place from the vantage point of the present (Johnsen and Olsen 1992:432). Thus, causality in analogy has an implicit ethnic perspective that has to be recognized when we confront ourselves with the question of who owns the past; otherwise, "our total reality is only an instant thick" (Willmot 1985:41).

In listening to stories told by native elders about the history of Greenland, we become aware of the widespread use of analogy in referring to archaeological remains, interpretations that correspond to our principles of connections in analogy (e.g., Gulløv and Kapel 1979–1980). In other words, the elements in ethnohistory are to be classified according to the relations between them that stress the continuity of recorded events. In archaeology, demonstrating similarity between source and subject inevitably dismisses any analogy between culture and history (Bateson 1972:153ff; Trigger 1991:563). In the Arctic North, it may be possible to penetrate the dialectical relation between the past and present and the discrepancies in the use of analogy in ethnohistory and archaeology because, from a cultural historical point of view, archaeology is inevitably a part of ethnohistory.

Stories from the Field

The year 1721 is a fixed point in the history of colonial Greenland. That year marked the return of Europeans to Greenland with the establishment of the Hope Colony, the first European presence following the disappearance of the Norse settlements. Eighteenth-century records attest to the difficulties faced by the European colonists and their inability to supplant indigenous social and subsistence systems with their own

European sense of order. Recognition of this fact by the colonists is interpreted as a concession to one form of Eskimo intellectual superiority. The traditional European conceptualization of Greenland's history has, therefore, been altered in favor of a more relativistic framework that puts Eskimo and European cultures on equal footing (Gulløv 1977).

In 1971, during the 250th anniversary of the founding of the Greenland colony, a discussion of intent and effect in history took place on Greenlandic terms based on the concepts of myth and symbol, as personified in the cleric Hans Egede, founder of the Hope Colony. On one hand, the colonial myth expressed the altruism of the colonizers. On the other hand, competing perceptions held Egede to be the symbol of colonial repression of Eskimo culture. This dichotomy, as expressed in the Greenlanders' attitude to their own history, became the basis for strategy and action and for rediscovery of their own identity. As a result of this discourse, archaeological research conducted at the Hope Colony sought to examine the objectives, observations, and criteria used to evaluate these alternative perceptions of the past—to explore “effect history,” as used in the sense of hermeneutics referring to tradition, semantic fields, and prejudices (Gulløv and Kapel 1979:207).

The ethnohistorical sources of colonial Greenland include stories about past triumphs of Eskimos in their encounters with Norsemen and whalers (Knuth 1968a; Rink 1866, 1871). These historical accounts have for centuries been the means of sustaining ethnic self-respect among the Greenlandic population (Gulløv 1985a:292). The written source material of Danish



8.1/ Kangeq village in 1972, showing midden area (center), and Apollo Thobiassen's house (front right)

origin (1721) and Moravian parish registers (1733) form important baselines for studies of historical ethnography. By using these, combined with oral traditions recorded in the mid-nineteenth century, an ethnohistory of West Greenland can be written.

My own interest in Inuit oral history came about through a chance encounter. In 1968, I was excavating at the Kangeq village in Southwest Greenland, a few kilometers from Hope Colony and the capital Godthåb. The Kangeq midden is approximately 3 meters thick (fig. 8.1). In the opinion of the project organizers, Helge Larsen and Jørgen Meldgaard, the site was thought to be comparable to Sermermiut in Disko Bay with a long record of Paleoeskimo cultures. One day when we removed the grass covering the top of the midden a number of tiny beads came to light scattered between pieces of crumbled wood. They appeared to be late-nineteenth-century Venetian glass trade beads and were thought to be associated

with the last habitation of the house ruins nearby. Among the local people watching our excavations that day was an elderly woman who suddenly laughed, clapped her hands and shouted, "You found my box!" In less time than it takes to tell, we were all transported back half a century to when she was a young woman and had just returned from the local shop where she had purchased a little wooden box with glass beads to be sewn to a collar. She lost the box on her way home. Although she had looked intensively for it, she had not been able to find it. Her house was abandoned in 1936.

This example shows how the archaeological recovery of beads and the speculation that they might have been used for decorating a woman's coat could be derived through analogy by assuming that the historical use of beads is mirrored by their present function. In this case, the woman's narrative, by means of a direct historical approach, confirmed formal analogy. From the narrative we also learned about the principle of connection between structures past and present in local memory. To archaeologists, the beads were just simple commodities; but in reality, they were part of local history. Today, it is easy to recall the definitions of formal and relational analogy (Wylie 1985:94-95) to describe the different uses of analogy

in archaeology and history, but in 1968, chance would have it that I then came to know about local Inuit history by intuition before I learned about ethnohistory in anthropological discourse.

The woman who identified the beads was married to Apollo Tobiassen (1907-1979; fig 8.2), the catechist who was also the local historian of Kangeq, and it was he who was responsible for the shift in my interest from Paleoeskimo archaeology to ethnohistory. Tobiassen's knowledge about past events was an invaluable contribution to the ethnohistory of Southwest Greenland. He could trace his family's lineage for more than 300 years back to the family's seventeenth-century communal houses that were contemporaneous with the Hope Colony!

The nearly three centuries of oral traditions that Tobiassen can draw from is the same period in which archaeological evidence reveals a discontinuity in the habitation of communal house sites; these sites appear to emerge suddenly, only to be abandoned a century later (Gulløv 1985b). On the other hand, the ethnohistorical sources inform us of continuity in the habitation of the region as people who lived permanently in the fjords moved out to their new winter quarters at the sites in question, and which led to the later settlement among the Europeans. Using informa-

tion from historical ethnography, we are now in a position to explain the uncertainty in the archaeological data, and furthermore to uncover the motives behind the changes within Eskimo society at that time (Gulløv 1985b, 1986).

Assumptions made about the past depend on the motives of archaeologists and historians. Accordingly, the history of Greenland encompasses a variety of ethnic dimensions embedded in both archaeology and in Inuit oral history. To uncover the motives behind the changes within Eskimo society is a challenge to our archaeological



8.2/ Apollo Tobiassen, catechist and local historian of Kangeq, with newly confirmed young Greenlanders in 1970

methods in recognizing our own inability to understand prehistoric features within a frame of instrumental reason (Johnsen and Olsen 1992:433). To begin, however, we can explore assumptions from the living oral traditions about subjective meaning in the minds of forefathers long dead when conducting archaeology as a source for ethnohistory.

Stories from the Past

From Apollo Tobiassen at Kangeq, we learned how the Kangermiut used the gullet from the Great Auk as a float for their bird darts. Great Auk bones were frequently encountered in the Kangeq midden excavations (Meldgaard 1988:172). Alcids, including the Great Auk, wintered in the ice-free waters along the coast of Southwest Greenland and served as important provisions for natives who came to settle in the area.

Among the newcomers we were told about was a certain Síngajik from southern Greenland who was the ancestor of our informant's family. He had arrived ten generations previously, yet in 1975 Tobiassen pointed out to us the ruin of Síngajik's first house on Hope Island, several kilometers west of Kangeq. According to the story, Síngajik moved into a little house where a widow lived with her only daughter, whom he married. Later, he enlarged the house to make room for his housemates and companions who had been with him on his travels from the south. The location of Síngajik's house was not typical of local Eskimo tradition, being some distance from the rest of the house sites at Illorpaat (Gulløv and Kapel 1979–80:353).

According to oral tradition, nobody subsequently lived at the site of Síngajik's house. Yet the continuity of the family story handed down over ten generations (perhaps 300 years) was substantiated by historical records (Knuth 1963). Tobiassen had learned the history of the place from a relative who died in 1925. Between the details in the narrative and the archaeological observations at the site, "formal analogy" provides insights into the events that took place at the end of the seventeenth century.

According to Gulløv and Kapel:

When the girl saw Síngajik come out of the house and go towards her, she went *up* towards their house to show him the way, as he followed behind. She had said that they lived *farthest south* and *uppermost* from all the others . . . by chance he happened to look out the window, only to discover the setting sun, its reddish glow already having spread across *the entire western horizon* . . . Patiently he sat, waiting, until the sun was finally so low in the sky, that it *shined in through the windows from the west* . . . This afternoon when I went *out of our house and looked down* . . . since I wasn't satisfied with just looking at them, I *went down to them* . . . (Gulløv and Kapel 1979–1980:374ff. Italics added.)

Archaeological investigations of the house subsequently uncovered additional details supporting the oral narrative. Excavation next to the large communal house, which was interpreted to have been used for one or two winters, revealed a small house with curved walls. In the entrance passage of the house was the body of a fifty-year old woman. Based on the large percentage of women's tools found in the small house, especially in comparison with the larger communal house structure, it appeared that the small structure had been primarily a woman's dwelling (Gulløv and Kapel 1979–1980:373).

Tobiassen's oral accounts contained additional information about the interior of the house and the grave. The story runs:

She pointed at *her daughter's side platform*, and Síngajik felt deeply thankful about this, because now he was to sit on *the window seat* just in front of the young girl . . . "It is true I am a woman, but I use her [the daughter's] help almost as if I was a man, and this is the reason why I have never given her away, though *the people who live just north of us* have often asked for her. Her help is the reason why we never, not even in the middle of winter, suffer need. But should it happen that she is given away, I prefer that the man who takes her will also live in this house. . . ." He [Síngajik] immediately started making arrangements in Kangeq, and already by the next day, *they* [the housemates from South Greenland] packed up and moved to Illorpaat. It was *no problem for them to obtain land, since they simply went straight up to the*

widow's house. . . . When Síngajik's mother-in-law died during their fourth winter in Illorpaat [according to the story], he wanted to move back to Kangeq. His wife looked at him and replied, "When I leave this place, I will not be able to stop thinking about my old mother, but she is the only one who binds my thoughts to this place". . . and her husband replied, "You must know that there will be times when your thoughts will be possessed by that which you cannot forget. You can then visit this place, *though it is only her grave you will go to. This you can do as often as you like.*" (ibid.:374ff. Italics added.)

The excavated faunal material showed the daughter was a good hunter. The 2,208 bone fragments identified from the oldest house amounted to 776 seals, 15 caribou, 28 dogs, 10 whales, and 1,379 birds, including 4 Great Auk, compared with the 2,218 bones identified from the youngest house, which represented 597 seals, 14 caribou, 28 dogs, 11 whales, 1 arctic fox, and 1,567 birds, including 3 Great Auk (ibid.:379).

Síngajik's move into the widow's house allowed him to gain access to land that was already inhabited. According to Eskimo traditional land ownership rights, permission to stay depended on local communal acceptance. Although it is an example of matrilocal residence, as mentioned in the narrative, this is, to my knowledge, the only case from Greenland, but it nevertheless enabled Síngajik to settle, and later, to enlarge the house to make room for his companions.

Clearly oral narratives have potential to provide insight into historical events. In several stories from Kangeq, Síngajik's descendants tell of the South Greenlanders traveling north to winter in communal houses (Gulløv 1987:84ff). Similar accounts are recorded in historical sources that provide information about travel activities along the west coast, including accounts of gathering places where hundreds of Eskimos would arrange to meet to exchange skins, soapstone, etc., for winter supplies, to search for marriage partners, and to exchange news (Gulløv 1985b, 1987). Reports written in the mid-eighteenth century describe territorial usufruct rights as practiced within native Greenland society (Brøsted 1986; Dalager 1915; Petersen 1963).

Of the many reasons for traveling, oral accounts testify to an interest in trading trips undertaken to obtain resources lacking in the home areas (e.g., baleen for fishing lines) or to obtain European commodities (Sonne 1990), and migrations to avoid blood revenge. The latter apparently was the reason why Síngajik first moved north. In former times, blood feuds had flourished between South and West Greenlanders but had been settled by marriage ties so that the South Greenlanders now had relatives and in-laws all over the coast (Glahn 1771:263).

Interpretations based on the excavation of early historic-period communal houses are in accord with historic ethnography in describing the wide range of activities, including shamanistic performances, undertaken in the winter dwelling (Gulløv 1988). The winter house served as a frame for the entire local community. The *qassi*, the men's house, became a physical part of the structure. "Thus the winter group could be considered as having once consisted of a kind of large house that was both a single and a multiple unit. This would explain the formation of settlements which were later reduced to a single house, such as at Angmagsalik" (Mauss and Beuchat 1979:47-48). The oral traditions and ethnographic analogy enable us to explain the shifts in settlement patterns that coincided with the environmental change brought on by a warmer climate (the culmination of the Little Ice Age), the changing resource procurement opportunities and strategies (e.g., increases in sea bird populations in the ice-free waters of southwestern Greenland), and with the increased involvement with Europeans (whalers and then colonizers whose presence widened the market of trade goods and transformed patterns of exchange behavior; Sonne 1990).

The historical situation of seventeenth- and eighteenth-century West Greenland thus parallels that of Labrador. "Herein lies a potentially fruitful avenue to pursue in reinterpreting the communal house phenomenon, one that is contingent upon drawing an analogy between the rules governing production, distribution,

and consumption of special trade items, those originating with the newcomers in southern Labrador" (Richling 1993:73-74; cf. Kaplan 1985:65).

With the advent of long-distance trading voyages, sometimes lasting two years, Greenlanders from distant areas would be allowed to settle briefly at local communities. These trading voyages are mentioned in both ethnohistorical and historical sources, including a voyage that occurred as late as 1900, when a group of Southeast Greenlanders moved to the west coast. Though the origin of these newcomers could be demonstrated by their distinct dialect (Schultz-Lorentzen 1904), the oral accounts of these journeys, because they lacked ethnographic and archaeological confirmation, were never critically accepted (Birket-Smith 1917:32; Gulløv 1982:13ff; Meldgaard 1977:40).

The recognition of oral tradition and linguistic studies as valid ethnohistoric sources (cf. Fortescue 1986; Petersen 1986; Schultz-Lorentzen 1904; Thalbitzer 1904) has afforded historians (cf. Gad 1984:64) valuable insights into the social organization of seventeenth-century Greenland.

An analogous situation occurred in eighteenth-century Labrador (Richling 1993) with the appearance of large multipurpose communal sod houses at Labrador Eskimo sites. In Labrador the communal houses emerged with the acceleration and intensification of whaling and trading relations brought on by the appearance of European fishermen and whalers in the Strait of Belle Isle. In spite of the relatively short period this house type existed in Inuit history, an analog to the European mission structure as both assembly and festival houses was pointed out when Thalbitzer in 1935 asked one of my informant's South Greenlandic relatives about the meaning of the *qassi* (Gulløv 1988:196f; Thalbitzer 1941:673). The analog established a principle of connection used in local history that is different from the ecological reasoning frequently employed in its archaeological expression, i.e., seeing the communal house as a response to climatic change (cf. Gulløv 1982; Richling 1993).

Analogy as Semiology

As discussed above, oral narratives and ethnohistory have the potential to greatly expand our knowledge of seventeenth- and eighteenth-century Eskimo society beyond that derived from archaeology alone. A second example is based on the problems that archaeologists have had in defining implement form and function, despite detailed descriptions of hunting equipment from both West and East Greenland (Dalager 1915:18; Thalbitzer 1914:323).

Collections of harpoon heads from the central-west coast, mostly from historic graves in the Disko Bay area, show considerable variation (Swenander 1906). In the archaeological record, variation is often attributable to the presence of distinct social groups. Given the oral narratives that testify to the great journeys sometimes undertaken by Eskimos, including one band that traveled halfway around Greenland from Ammassalik to Disko Bay in two years (Gulløv 1982), it is not unreasonable to suppose that some portion of this variability is attributable to the movement of hunters between different regions.

Among the customs brought to the west from the east coast where many people lived as noted in 1733 (Egede 1925:267), we learn about a tradition from Aron of Kangeq, an ancestor of Apollo Tobiassen (Gulløv 1986:173). Aron's story, situated in South Greenland, tells about a hunter from the east coast who died on a trading trip around Cape Farewell. Discussing the burial custom with the local west Greenlanders, the sister of the dead hunter declared: "If we bury him on the ground his soul will suffer from cold and distress. We will therefore follow our custom and lower him into the sea where neither cold nor lack of food exists," and they lowered him into the sea together with his tools and equipment (Meldgaard 1982:66). We have no analogs from the west coast to the burial custom described here, but it is well known in the nineteenth-century literature about the east coast.

The stories told through the centuries in Kangeq among Tobiassen's ancestors bear witness to a real

world where objects found by the archaeologists are subjects in Inuit history. To use analogies is a part of the ethno-historical work, just as it is to the Eskimo narrator. We have to define both sides of the analogy from an ethnic point of view, i.e., the ethnographic source and the archaeological subject, in order to demonstrate similarities, while to Tobiassen and his relatives an object is a sign of something that existed in their history. Thus, in general, a sign consists of three components: its appearance as a type (e.g., a winter structure), the object to which the type refers (e.g., a communal house), and the interpreter, the individual or collective that interprets the relationship between type and object (e.g., stories about trading expeditions and travels) (Guiraud 1971:49, 55ff). From an anthropological point of view, the interpretation of a sign follows a semi-

ological practice in which one searches for the meaning of the code chosen rather than the meaning of the encoded message (Bateson 1972:130). The interpretation of glass beads, the communal house as a *qassi*, and the burial custom as signs analogous to events in Inuit history make sense to local informants in a way different from our "translation." Using analogy as semiology we can accomplish more comprehensive ethno-historical work. The following example illustrates this semiological approach.

In 1918, Birket-Smith excavated some eighteenth-century communal houses at Ikarassanguaq in the Egedesminde district from "the period immediately preceding and coinciding with the activity of Hans Egede, when the Danish colonization proper was as yet in



8.3/ Harpoon heads found in Egedesminde (top) by Birket-Smith have similarities with this harpoon head from Ammassalik, a poorly explained archaeological fact.

its infancy" (Birket-Smith 1924:46). Among the artifacts found were harpoon heads used for sealing. They were unbarbed and had attached endblades and two dorsal spurs. They were similar to sealing harpoons recovered from East Greenland.

"The Ammassalik type of harpoon head with basal barbs [i.e., spurs] facing each other and without marginal barbs is not limited to this district, but it is noteworthy that there is no other district, so far as we know, where this type has become predominant to such an extent over the other types as here" (Thalbitzer 1914:430, fig.133e).

The spurs of the heads found by Birket-Smith at Ikarassanguaq have slightly clefted points, a feature observed on similar harpoon heads from that period in

Disko Bay and on other types from early post-contact sites (Gulløv and Kapel 1979:75; Swenander 1906: plate 2). In other words, we have a harpoon head type that belongs to one group common in seventeenth- and eighteenth-century central West Greenland and Ammassalik (fig. 8.3). On typological grounds, the western and eastern types are analogous to a certain degree, but on the ventral and dorsal sides of the harpoon head found in the communal house two grooves are to be seen crossing, as well as two converging lines between the apertures of line groove. On historical grounds we can exclude the possibility that the types represent a classic example of parallel cultural evolution because, according to the eighteenth-century sources, contact was already established between the east and west coast.

From ethnohistory we are aware of the activities that had taken place in the region in question where Greenlanders from south and east wintered in communal houses and during the eighteenth century settled in the region. They spoke another dialect, and it should be stressed that influences from East Greenland grow less and less in West Greenlandic dialects the farther north one travels from Cape Farewell.

Now, intonational features resulting from substrate influence tend to be rather long-lasting, and it is linguistically quite in order to suppose that the special accent at Aasiaat (Egedesminde) may, indeed, represent the last trace of eastern influence as it moved up the west coast. . . . In other words, it is highly likely that there was a southern element in the makeup of the original population of Aasiaat (Egedesminde), and that the phenomenon in question . . . is indeed the northernmost trace of influence from the south. It probably did not come from Upernavik, far to the north of Sermermiut. Intonation is an important tag for group identity, and intrusive features of this sort would have met with greater resistance (perhaps even conscious resistance) in more densely populated areas such as the southwestern deep fjord area around Qaqortoq (Julianehaab). On the other hand, certain segmental features emanating from the east were not so effectively resisted here, as we have seen (Fortescue 1986:420ff).

Returning to the 1918 excavation, the harpoon head from the Egedesminde district is a clue to the explanation of the far-reaching travels and trading expeditions, and the ornamental carvings used on the head call attention to the artistic capacity of nineteenth-century East Greenland (cf. Thalbitzer 1914:616ff). Using a semiological approach to this artifact found in a well-defined historical context, we have to incorporate the three components of the sign described above as a basic unit of relationship. To say that the harpoon head is a type commonly used for hunting smaller seals in certain regions on the east and west coasts where similar types have been found is only a tautology. The inclusion of the interpretation of the semiological relationship between type and object (i.e., between design and sealing) as given in the ethnohistorical sources enables us to analyze the meaning of the code chosen. The interpretation of the relationship in the eighteenth century relies on religious beliefs in which the Sea Woman plays a decisive role in hunting and trade—"Implements were attached to the hunter as the soul to the body" (Rink 1871:176; Sonne 1990:30)—and on social relationships and aesthetical expressions. These are connected to expressions of East Greenlandic group identity (Fortescue 1986; Gulløv 1982:13ff).

From the semiological approach (Guiraud 1971: 55ff), the harpoon head communicates an East Greenlandic origin. To the archaeologist, the object is an element of ethnohistory that tells of past events. Taking into consideration the circumstances of the eighteenth-century west coast, we have an analog to nineteenth-century East Greenland where the changes from outside also influenced material culture. However, the search for the meaning of messages encoded in material culture of East Greenland is difficult because analogical reasoning also involves a Dorset culture contact not yet proven in this part of Greenland. On the other hand, a late cultural influence from the north, as mentioned by Thalbitzer from oral traditions, seems possible according to new investigations in archaeology and linguistics (Gulløv 1995; Thalbitzer 1914:346).

The use of analogies taken from historical ethnography and oral traditions in South Greenland have been demonstrated. The method is quite different from the one used by Therkel Mathiasen. Although he found a historic reality in the Eskimo traditions of the Canadian Arctic, he never used this knowledge when he excavated in Greenland later on. Prehistory is progressive, and the archaeologi-

cal reconstruction of past lifeways includes analogies, which makes it different from ethnohistory where traditions can be followed back through the years. By combining these methods, one gains new insights that, in the future, may illuminate for example, the Dorset-Thule succession expressed in the harpoon head from Egedesminde, which has a similar late Dorset decoration (Maxwell 1985:160).

Northern Ellesmere Island: A 1958 Field Diary

MOREAU S. MAXWELL

In the summer of 1958, the Defense Research Board of Canada elected to send an extensive research party to northern Ellesmere Island in Canada as a contribution to the worldwide International Geophysical Year. It was planned to include an archaeologist among the eighteen scientists in the expedition. The logical choices were either Scotty MacNeish or William E. Taylor, Jr., both of the Human History Branch at the National Museum of Canada. As it happened, both had previous commitments for the summer, and to my lasting pleasure Bill Taylor asked if I would represent the Museum in this venture. It was an ideal opportunity for me. I had spent the previous three winters on the sea ice along the 70° parallel, locating ice landing strips for cargo planes for the Distant Early Warning line, and had become intrigued by the Arctic. No professional archaeologist had yet been to northern Ellesmere, and perhaps it held answers to many of our questions.

My mission was to survey on foot as much of the terrain as possible and, where feasible, to excavate such sites as I found. I was helped in many ways by some of the other seventeen scientists at Lake Hazen, although, as will be clear below, I spent little time with many of them at our base camp. Geoffrey Hattersley-Smith, a glaciologist with the Defense Research Board was in charge of the expedition. Roger Deane, a limnologist with the Defense Research Board was next in command; other members included Robert Christie and Barry Walker of the Geological Survey of Canada, David Ingle Smith and John Powell of McGill University in meteorology and botany, John Tener of the Canadian

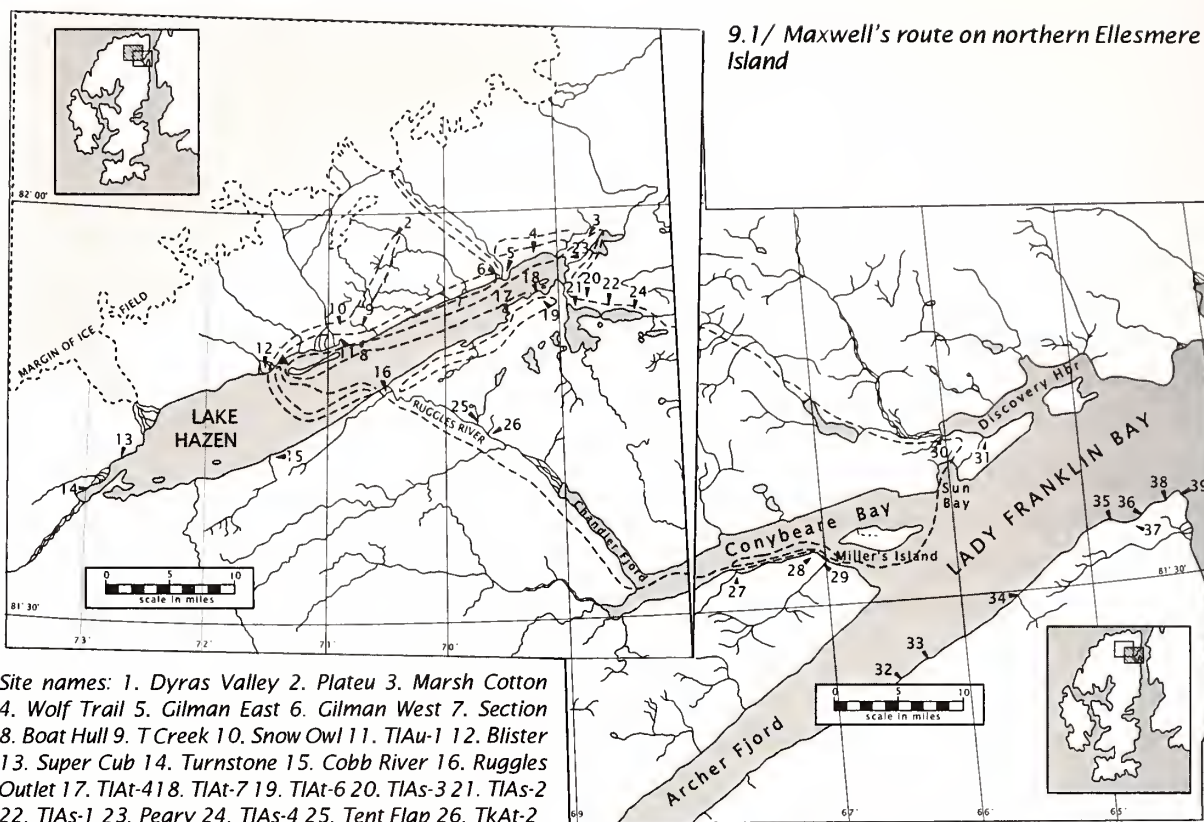


Wildlife Service with an interest in musk oxen, James Soper in botany, Michel Brochu of the Geographical Branch, Department of Mines, Ian McLaren of the Fisheries Research Board, Keith Arnold the surveyor, and Helmuth Sandstrom in geophysics.

Summer of 1958

During the summer, I kept an archaeological journal, which went to the National Museum. I also kept a personal diary, which I am making public here for the first time. It has some historical interest in that it records the first archaeological exploration of the region and marks the beginning of my thirty-five-year involvement with arctic archaeology. The text that follows is extracts from that diary, much of which is too long, wordy, and personal, and so I have abbreviated it in parts and elaborated it in other sections for the purpose of this article.

Saturday, May 24. I arrived in Ottawa where I began an excellent four-day briefing on arctic archaeology by Bill Taylor. He patiently and expertly reviewed for me virtually all that was known to date on arctic prehistory.



Wednesday, May 28. The party departed by plane for Winnipeg and an overnight at Churchill. The next day we enplaned for Thule Air Base, Greenland, with a stop at Resolute and landed on the lake ice of Lake Hazen, Ellesmere Island, in a C119 at 0100 the morning of Friday, May 30 (fig. 9.1).

Friday, May 30. I settled into base camp in the north shore of the lake and then checked out the foothills on skis. Everything was snow covered, no observable rock features. Peculiar sensation to have sun directly north at midnight.

Saturday, May 31. With Dingle (David Ingle Smith) set off on skis up the Gilman Valley with supplies for the research team on the glacier. On the valley delta were three small tent rings showing through the snow. Snow too sticky for skis and we abandoned them. Constant up grade of 110 feet per mile. Often broke through shell of ice and sank to crotch. When we finally reached the glacier face fourteen miles above the lake I was exhausted and had badly strained my right tendon. This gave me trouble through the summer.

Sunday, June 1. By 0200 Geoffrey Hattersley-Smith and two others from the glacier crew had joined us. With five in the tent, it was hot and stinking and I moved to finish my sleep in the snow. Woke up about 1000 with a white fox three feet away staring at me. About 1200, Bob Christie, Barry Walker, and their dog team came tumbling down the glacier face and we all packed up for the trek back to camp, arriving at 0300.

Monday, June 2. Loafed in camp.

Tuesday, June 3. Searched six miles of the delta west of the base camp—nothing.

Wednesday, June 4. Crossed Lake Hazen with Bob Christie, Barry Walker, and a nine-dog-sled team, the three men skiing beside the sled. The plan is to go down the Ruggles Valley to the coast where I will camp and search for sites. Started down a narrow ice foot along open water. Fast current with a number of whirlpools where the water goes in and out of ice tunnels. Very cold and windy. As we traveled down the valley, we saw five musk oxen on different bluffs

west of the valley. At about 2000, we stopped ten miles down the valley where Bob had previously seen some rock features.

Thursday, June 5. Investigated circular rock cache on top of gravel terrace and what appears to be a rectangular tent ring due north of it. With snow on the ground, I was unable to locate any cultural material. (I believe that this cluster of rock features is the one that Pat Sutherland in a revisit in 1988 identified as an Independence site.) I checked the bluffs on both sides of the valley for nineteen miles south of Lake Hazen without seeing signs of features. Camped at the head of Chandler Fiord on Shell Point. Number of raised beach strands with fossil shells but no signs of prehistoric habitation.

Friday, June 6. Searched the shoreline about two miles east and west of Shell Point without success. Sledged along north shore of Coneybeare Bay. Steep talus slopes both sides of bay—no possibility of camping spots. Crossing Coneybeare Bay to Tent Ring Creek where Bob, in 1957, had seen a number of tent rings. Counted ten small tent rings barely visible about the snow. Will revisit later in summer. Fox barking on hill above while we set up camp.

Saturday, June 7. Cleared snow off of most of rock features and one-half inch of moss and dirt off the flagstone floor of House 1 but found nothing. Prepared cache of food for my return here. Plans for party: Bob and Barry leave tomorrow for Carl Ritter Bay. Plan to return to Miller's Creek, where I will be camping, on or about June 18. If they haven't arrived by June 22 I will walk back up the Ruggles to base camp or overland from Eastwind Bay.

Left Tent Ring Creek at 1745 for easy eight miles to Miller's Creek. The sled dog Lonesome took the lead for the first time and did very well . . . Miller's Creek renamed in honor of Lonesome. Now Lonesome Creek. Site looks excellent. Old delta with several tiers of raised beaches with structures on all except the top terrace. Located just across from Miller's Island with view down Coneybeare toward Fort Conger.

Sunday, June 8. Awakened to a warm, 41° Fahrenheit day. Great swarm of large flies—like green bottle flies. Swarmed over dogs and everything. Mating everywhere and immediately laying eggs. Got into freeze-dried beef and laid eggs in every hole. (This was the first experimental freeze-dried beef and it was perforated all over its surfaces.) For a while I tried to dig out all the eggs but then gave up and ate them along with the beef. Helped Bob and Barry pack. They left at 1300. Set up my camp after they left. Seems strange here alone. Strong feeling of lassitude hit me and I just dragged along. Every noise seems magnified—the slumping of snow on the sea ice, the far off rumble of rock slides on Miller's Island and on the north shore of Coneybeare Bay. Crawled into the sack at 2000—now below freezing and has been since 1800 although day still very bright, clear and calm.

Monday, June 9. Up at 0730 and out to dig at 0800. Cleared snow from Feature 1 and Feature 2. Well-defined fireplace in Feature 1 but no artifacts. Had the impression that time would go faster if I could just find an artifact. Time seems to drag very slowly. Flies very bad today—all over everything. Feel that the two walls of F1 may be just a windbreak around the fireplace. Ate at 1800 then back to work until 2130 when I had my big meal. Sky overcast and very cold. Slight breeze. Read until 2400.

Tuesday, June 10. Awakened with awful start about 0600 as left side of tent fell in. It was so sudden I felt sure an animal had done it. (Before I had left base camp on Lake Hazen, Geoffrey Hattersley-Smith, worried that I would have no means of protection while camped alone on the coast, had loaned me his father's old military issue Webley .455). Thinking it was a bear, I grabbed the Webley, unzipped the front of the tent—still zipped up in my sleeping bag—and found myself staring at a white wolf standing by my food box less than four feet away. I shouted and he ran about 10 feet, then stopped and stood looking at me. I got out of the bag and outside in my sock feet. I shouted again and threw some rocks—inaccurately. Each time he

bolted about 10 feet. Apparently he has no fear of me. [There follows about two full pages of the journal describing the wolf.] I went back up to my dig and he lay down on a sunny gravel patch and watched me trowel from about fifty feet away until late afternoon when he sauntered over the highest ridge. In assessing the damage, I found that he had simply dragged cloth bags of dehydrated food around without eating any (speaks to the tastiness of dehydrated food) and had eaten only the leather strap on my ski pole. Unfortunately, he had rolled in the sated dog's manure and then rolled on the cloth food bags. (By the end of the summer this hardly bothered me.) In a way it seems nice to have company even if it is a wolf. Got miserably cold and came in (tent) for hot soup and coffee at 1500 (I had very little gasoline and could only burn my little tin-can stove long enough to heat water for meals). Back to work at 1600. Almost finished excavating Feature 1 and mapped it—not one blasted artifact. Gets very discouraging—looks like a good site but this is an awful lot to put up with for negative returns. Fox has been laughing from hill above Lonesome Creek. Most peculiar sound I ever heard. Cross between baby wailing, hyena laughing, donkey braying and dog barking. Mountains a lovely cold shade of blue and white. Barren land. Ugly yet it is beautiful. Cached surplus gear in huge rock cairn . . . turned in at 2300.

Wednesday, June 11. Amaguk the white ghost of Lonesome Creek is back. I woke up suddenly and saw it was 0330. I figured it must be something and I listened intently. It is strange how few and yet how many noises there are when you are alone in the wilderness. There are the far off rumbles of rock slides that go on continuously from Miller's Island and the slopes of Coneybeare Bay. They sound like a far off jet plane or the stomach rumblings of a giant. During the day they are nearly continuous but slack off during the night. The flies buzz constantly with the higher pitched zz of a mating pair. The melting of the snow hummocks near the tidal crack slough like footsteps and the rustle of the tent also sounds like something stealing up on

me. I have even been fooled by my stomach growling or a little catch in my windpipe. The birds don't sing as much at night nor do the foxes bark. There is a distant diurnal change in noises and impressions. It gets colder at 1800, although the sun does not seem farther away, and begins to warm again at midnight. To get back to this morning. I heard a slight rustle in front of the tent. I got out of my bag and unzipped the front. There was the white devil. The wolf had dug up my cache and dragged everything around including my precious can of gasoline. Losing any of my scarce supplies could have serious consequences. Reluctantly, I decided the wolf must go. I shouted at him and threw rocks but he only ran off a little way. Finally I got out the Webley .455 and shot in the air. It made a tremendous boom but he only ran off a short distance; so I shot at the snow beneath his feet. He yelped (the only noise I ever heard from him) and jumped straight up in the air. He turned several times and looked back but walked steadily over the brow of Lonesome Creek. I assessed the damage. He had eaten my prized stinky cheese; rolled on my bar of soap—getting dog manure on it—and dragged around practically everything, but without really destroying anything. Went back to sleep about 0400 until 0800. Must find something today to make all this worthwhile. These either the most impoverished Tunit or the most careful. Today the warmest yet—worked all day in sweatshirt and T shirt. The moss is starting to bloom—little tiny reddish purple blossoms. And some of the willows have pussy willows. About 2030 a great sighing like a strong wind from up Lonesome Creek. Gained in intensity, and died and gained again. Guess it must be the ice breaking up from the creek but cannot see into the gorge from here. Spring has really come today. The snow has just sloughed and slewed away. None left on my gravel shingle now. Turned in at 2300.

Thursday, June 12. The sighing up Lonesome Creek gained in intensity—like a train rushing through a tunnel. Often a sandstone slab lets go and comes ringing down the bluff. Had lunch at 1400 and had just come

in to warm up and have a cup of coffee and a cigarette at 1830. The sound last night and today has been the melt water rushing down Lonesome Creek. Spring has been in the air. Lots more birds—two more snow geese. Tidal pools are forming on the sea ice and so much snow has gone from the site it is hard to recognize features that looked so clear before. Put in a hard day at the dig and finally came up with tangible results. Found a broken knife handle and a piece of drilled whale bone in F2. Just noticed as I came in that Amaguk had chewed off three of the back guy ropes to my tent. Those back tent ropes are just inches from my head when I am asleep. Quit work at 2130—bitter cold wind goes right through you. Hands were numb.

Friday, June 13. Read and worked on notes until 0300, then slept until 0930. Beautiful day today. Warm and calm. Finished excavating and mapping Complex 1. Found a piece of cut antler. I am forced to conclude that this is just a camping ground—most of the circles appear to be meat caches. The little stone boxes are peculiar (they were fox traps). Too bad, with this opportunity not to be a really productive site.

Saturday, June 14. Day warm and calm. Slept well last night. Decided to inspect the reefs offshore between the delta and Miller's Island. Didn't really expect to find anything but knew I would always regret it if I didn't look. (Did look—no sign of occupation.) Started excavation on pit house (Feature 11). Appears to have sleeping platform at one end—paved with pieces of slate. Had good stew at lunch/dinner at 1600 then back to dig. About 2000 heard wolf fight to the east . . . went to tent for field glasses and saw it was Bob, Barry and the dog team. They had run into rough ice and hadn't made it to Carl Ritter Bay. Had gotten no farther than Cape Baird with backpacking. They had found Brainerd's petrified forest and place where Greely had cached his boat. Seems mighty good to talk to someone again.

Sunday, June 15. Bob, Barry, and I surveyed the site, running in the centerline and baseline. Excavating F11

down to sleeping platform. Good to have company in camp again . . . Beautiful day today, warm, few flies. Will turn in early, 2030, from look of sky may be cold and windy tomorrow.

Monday, June 16. Miserable night last night. Very high winds. My little tent was ballooning out like an airplane hangar and the skirts kept flapping up. Apparently some blowing snow in the night. . . . the cold went right through you. Bob and Barry wisely decided to stay in camp another day. Finished surveying the site and mapped in 27 features. Cold wet work. Finished excavating F12 (pit house)—no artifacts at all. Have been out of cigarettes for two days now and almost out of pipe tobacco.

Tuesday, June 17. Bob and Barry left about 1430 taking five dogs and the sled with them and leaving four of the dogs with me. They howled until the others were out of smell range and three of them settled down. Droopy kept chewing on his tie-down and about 1745 worked his way loose. I tried to lure him with pemmican but when he found the sled tracks he was off like a shot after the others. Worked on pithouse F11 all day—permafrost in patches all through. Had just gotten to floor and found two drilled fox teeth. The boys left the Mount Logan tent and the Coleman stove so I work and eat here and will sleep in the little orange job. I put rocks all around the skirt so it should hold. Coldest day yet. Here in the Mt. Logan my hands are numb and my breath makes a great steam. One of these days I have to go back down to Tent Ring Creek to map the site, do some excavating and pick up the food and gasoline I left there. Don't look forward to it. It will be a cold wet trip. Kind of bad to be alone again but not as bad as last time . . . Boys will be back June 24 or before.

Wednesday, June 18. Awful penetrating cold last night. Wind, low clouds, no sun. Must be the humidity. The wind across the sea ice picks up all the moisture from the melting snow. Put in a long hard day—ten hours on the house and four hours in surface search now that the snow is gone. It was worth

it—found my first harpoon head. Weathered and old but a great thrill (a very late Thule/Inugsuk type), also a large piece of drilled whale bone and two pieces of cut antler. Beginning to seem worthwhile. Don't feel like going to bed—beautiful night out—only difference between day and nights, the night is slightly cooler, the sunlight is not quite as strong and the sun is in the north. Even the dogs are restless and antsy tonight. Feel like I have been at Lonesome Creek forever.

Thursday, June 19. Slept late, then put in a hard twelve-hour day. Still nothing in F11 but a lot of interesting structure. Found whole new complex of tent rings on east side toward creek. Large, Thule type. Found cute little wooden doll—couldn't help think of the father who made it. Also drilled pieces of bone, wood and musk ox horn and of cut antler. In the evening, located a real midden on the gravel bluff just west of centerline. Solid bone just under the sod. Found bladder dart point in midden. Took walk over to creek—really roaring now. Dug up all old cigarette butts from the gravel and rolled a smoke—tasted wonderful. Saw first bumble bee.

Friday, June 20. Awfully cold and damp this morning but above freezing. Stiff in all joints. Can't seem to warm up. Scratched on midden for about an hour and a half, then in out of the wind for coffee; back out for an hour and in for hot chocolate. First time found a broken arrow point; second time a broken ivory button. Toward evening went over to C6 to look for whisk broom which wolf had dragged off the other day. Outside F31 found harpoon socket with piece of harpoon still in it—just lying on the ground. Turning over rock inside, saw nothing. Turned over next rock, here were three amber beads and a chunk of amber. Looked back at first rock and sure enough, two beads I hadn't seen. Then another rock and a beautiful harpoon head. Cold clear through but a good day. Out toward Cape Baird and north toward Lake Hazen the weather always looks clear. This spot seems to be a bowl for bad weather.

Saturday, June 21. Still cold and penetrating. Breath steaming. Freezing rain last night. Will try to get a lot done then feed dogs early and strike out for Tent Ring Creek. Intermittent snow. Continued work in F31. Found a total of nineteen amber beads and one ivory one. Decided to hike down a mile or so toward Tent Ring Creek to see whether I would need skis. On a gravel shingle about a mile and a half down I found a drilled soapstone mending sherd and then the rest of the pot sitting on the gravel. Returned to Lonesome and found a seal *ulna ajaqaq* (a cup and pin game) and a bone arrow head. Really been my lucky day. Slight drizzle. Set off at 2100 for Tent Ring Creek eight miles away.

Sunday, June 22. Really pleasant walking and skiing. Alternately moved from the ice shelf along the shore and out to beyond the hummocky zone. Many tide cracks. Around a point a fox, now in black and tan summer coat, came right up to me. We talked for a while. He walked along for a while about ten feet away. Decided to cross from cape to cape. The ice covered with one to four inches of melt water. Feet quickly soaked. Many seal on the ice. Checked every potential shore site. Feet dead and numbly sore, shoulders ached from the pack. As I got to the site, I saw a wolf up on the crest of the hill. Thought it was probably the one from Lonesome Creek. Many signs of wolf around—had scratched at but not damaged my cache. Piled up a few rocks and spread tent fly over them. Went to the bag about 0200. Couldn't sleep—too tired, too cold, ground too hard, too worried about being pounced on by wolf. Dozed off and on. Never more than an hour. Had to get up four times. Feet so sore could hardly stand on them.

Monday, June 23. Finally got up at 0700. Looked site over; sketched structures and looked for artifacts. Found only one unworked bear canine. Started for home at 1100. Feet and legs awfully stiff and sore. Feet probably have a touch of trench foot. Arrived back at camp, fed dogs, had a big meal at 2300, and dozed off.

Tuesday, June 24. Woke up refreshed and ready for work at 0730. Found six or seven good beads and

two nice lumps of amber from F31. Found multi-drilled bone strip—decoration for possible wooden vessel, and another just like it with a handmade iron nail stuck in it, and a long section of whale bone seal probe all from F33. Found more beads in other tent rings. Surprisingly, no artifacts or even food bones in the pithouses. Sun out for ten minutes. First time in ten days. Boys should be back tomorrow.

Wednesday, June 25. Still cold. In afternoon climbed up the peak behind camp. At the fifty-foot level, I found what I thought was a badly eroded fox trap. (When I worked with Eigil Knuth in Greenland later in the summer, I realized that this was probably the remains of an Independence house. Later, in 1966, Eigil confirmed that it was.) Saw the boys around the bend at 1830. They had been miserably cold too. No gas in the cache at Archer Fjord. Have to get gas by tomorrow. Guess we will leave for Sun Bay then up to Musk Ox Bay for the cache. Sure glad to have someone to talk to again. It was really amazing—just as they hit the melt pools around Lonesome Creek a big old *oogruk* (bearded seal) stuck his head up. Immediately all the dogs jumped in the water and nearly dragged the sled in too. The seal then flipped and played around in the water putting on a real show. It was just as if he had been waiting for a dog sled.

Thursday, June 26. Cleaned up work at Lonesome Creek and off for Sun Bay about 1500—arrived at 2100—13 miles. The snow melt left water four- to six-inches deep on the snow ice and our feet were wet and cold when we arrived. Sun Bay was disappointing. It was muddy and silty and hard for us to find a dry spot for our camp. Greely's account had mentioned Eskimo sites here but there was no possibility of them. Think we will try to haul the sled across the mud to Basil Norris Bay tomorrow then up to Fort Conger.

Friday, June 27. Up at 0600. Others still sleeping. Walked 6 miles around east side of the bay then across to Stony Cape. Climbed up on cape to look for cache supposedly left by Coneybeare. Since we were out of dog food and almost out of gasoline and people food,

Bob decided to skip Fort Conger and to start up Black Rock Vale tomorrow. I decided to cross from Sun Bay to the upper part of Discovery Harbour. Half way back, in Basil Norris Bay, I found the site Greely had described and where apparently he had found a Dorset harpoon head. The Dorset house was there but virtually taken apart. At the edge of the house I found a Dorset fish spear. This site well studied by Greely. I put in 18 miles today, winding up at 2230 and feel great. Plan 18 miles tomorrow up Black Rock Vale.

Saturday, June 28. Took off from Sun Bay for Lake Hazen, backpacking and dog packing up Black Rock Vale. So far as we know we are the first non-Eskimo to walk up this valley since Greely and two of his men made the trip in July 1882. The first two miles were over awful mud flats, then a mile or so of sand dunes, followed by a steep rocky slope that opened out into an interminable gravel flat. We followed the muddy shore of Lake Heinzelman for eight miles but saw no sign of prehistoric occupation. Finally made camp at turn in the valley at 2100. Legs and feet very sore. Chow and warm tent felt good. Beautiful view of Lake Heinzelman back in distance. Slept on gravel terrace with slight slope. Slept with head downhill to relieve legs. Climbed in sack with wet socks and wet underwear to dry them.

Sunday, June 29. Woke with feet, legs and shoulders feeling pretty good. Running short on food and fuel but should get to cache tonight. Just before turn in river valley found old camp of Greely's. Pieces of stovepipe, heavy tin cans and a large, very heavy baking pan. Now midnight. Walked until I thought I couldn't walk another step. Just slog along through the mud, rocks or creeks, soaking wet, stiff, sore and tired. About nighttime we came into the lakes and knew we were close. Found a good camp site about five miles south of the cache. Barry and I made camp while Bob took the dogs, who had only been fed once in three days, up to the cache. We made stew out of the last meat bar, egg powder, and dried onions—delicious. Out of coffee or tea. Lots of snow geese here on Lake

Beiderbeck. Plan to look for tent rings and meet Bob halfway tomorrow.

Monday, June 30. Up at 0600 and inspected two tent rings on small peninsula in lake—nothing exciting. At 1000 started to pack when Bob returned. He said there was a welcoming party five miles north at the cache. Met John Tener, Jim Soper, Michel Brochu, and John Powell who had come to take us back to base camp by J5 tractor and sled along the lake ice. Now back in camp (2000) after cold trip followed by many cigarettes, drinks, good food and warmth. Amazing how civilized and comfortable this grubby little camp can be.

Monday, June 30. This is really Monday the 30th. I must have screwed up on dates when I camped alone. This was to be my day loafing in camp. Instead turned out to be the roughest one yet. Roger Deane and Ian McLaren were going to take the J5 tractor to the mouth of the Ruggles River. It seemed like a good opportunity to get a ride there to excavate the house I had seen back on June 4. The ice had melted some distance back from the shore, so I took a small rubber raft to get in to shore. Roger and Ian returned to base camp, and once more I was alone. This is an amazing spot. Everyone passing throughout the Hazen Valley had camped here. Greely and some of his men had dug a little bit in the Thule-type house in the summer of 1883. Raining quite hard; so ate, then out in the rain to scratch around and walked three miles south and two miles west; then back again. Warm and still raining hard. About midnight at least 200 feet of ice had disappeared out from the shore and it looked weak all the way out to the middle. The lake had been constantly candling (the freshwater-lake ice had formed vertical crystals two feet long, which were too strong to push a boat through but too weak to support any weight), and I felt that if I didn't make a try at getting back to base camp eight miles across the lake I would be stuck there alone again for three weeks until a boat could get through. I packed up wet tent, stripped to cotton underwear, put anorak over that then forced

the dinghy into the candled ice as far as it would go. Then with oar to help hold me up, slid on my belly to firmer ice. Sunk ice ax, tied rope to it and went back for gear. Had to haul gear by rope—ice wouldn't hold up under both gear and me. When got to firmer ice, tied dinghy to outside of pack and started off. I walked all night with occasionally one leg and then the other falling through the ice and struggling to stand up. Finally reached camp at 0500 and fell into bed. The next day I weighed my pack when things had dried off some and it weighed 82 pounds. Mosquitos out for the first time.

Tuesday, July 1. Slept until noon and really loafed in camp. Today is Dominion Day, so put on bow tie and shaved my side whiskers. Brought the archaeological catalog up to date. Ice on this side is firmer so tomorrow Bob, Barry, and I will take the tractor to the east end of the lake where I will camp for about three weeks until the lake opens up and someone can pick us up by boat.

Wednesday, July 2. Went over all my gear in preparation for trip; then from noon to 1600 walked about six miles in search of tent rings on delta of Blister Creek—west of John's Island. Left base camp at 2100 with J5 and sled. Tractor stopped twice on way and we had to suck out fuel line. Got to the end of the lake, but there was open water for about 200 feet to shore. Had to jump in the shallow water and carry the gear ashore in many trips. Candled ice cracked against shins. All gear finally on shore. Finally had tent up and coffee brewing by 0500.

Thursday, July 3. All slept until about 1800. Warm and mosquitos fierce. Went to see the reported site. These were all modern Eskimo houses apparently built by Peary's men in the winter of 1906. Lot of metal, cloth, and tin cans around small stone houses. Chunks of amber weathering out of coal seam on beach.

Friday, July 4. (There followed twenty-four mainly frustrated days. The mosquitos were bad, the weather fluctuated from very warm to very cold, and there were no signs of prehistoric occupation on this muddy and

boggy end of the lake.) Mosquitos so bad nearly drive you out of your mind. Great cloud around head, fly in ears mouth, nose and eyes. (In subsequent years, I grew to expect mosquitos in the arctic summer but they have never been as fierce as they were that summer at the east end of the lake.) Had last drink out of the bottle to celebrate the fourth.

Saturday, July 5. The ground here is about two feet of moss, grass and silt over a three-foot fossil ice lens, which overlies a seam of coal from which amber nuggets are weathering. This area is called Turnabout Creek. Very warm in early evening—probably high forties, low fifties—thermometer broken. Five musk oxen between Turnabout Creek and Salor Creek. Peculiar echo phenomenon from cold dense air mass over lake ice about 50 yards from shore. Searched beach, lot of tin can stoves, whip handle, wooden upstander, etc.

Sunday, July 6. Bob and Barry took off at 1400 for a fourteen-day dog and back pack to the northeast. I searched the height of land for six miles around the end of the lake—nothing. Little yellow poppies and purple saxifrage all over hills. Well, I wanted to get rid of the bugs and I did. About 2300 a storm blew up—and what a storm. Rain, wind gusting from estimate of 40 to 70. Tent filled with sand. Bitter cold, tent crashing and billowing—never heard such a racket.

Monday, July 7. Happy Birthday to me—what a day. Wind has not abated one bit since eleven last night. The wind continued through the day. Sun was out and it was too hot to sleep in the tent and too cold outside. Now 0400, not the slightest bit sleepy—really off schedule.

Tuesday, July 8. Finally dozed off about 0600 and slept until 1200. Wind died down a little. Packed backpack and set off for the lakes at the head of Turnabout Creek. Really enjoyable, beautiful day, strong breeze. Walked four miles east along the shore of the lake—pretty, but all silt—no Eskimo signs. Stopped at 1700 for sardines and oatmeal bar—wind died at last—after 42 hours. Varmints up immediately in great blood-thirsty swarms. Back in tent at 2000. Will fix good

meal of spaghetti and cheese and carrots. At least my feet and hands are no longer swollen and sore and my right heel tendon is almost completely cured.

Thursday, July 9. Awoke at 0800—glad to be back on schedule. Plan to take overnight hike to the Gilman River today. First will try to take bath in washbowl. Decided I better sketch the house nearest the lake. Much the same design as other houses I have done but has a deeper cold trap entrance. House covered with canvas tent—musk ox skins on sleeping platform, old kamiks (boots), iron toggle rings, pointed iron rod, tin can, tin bowl, several small chunks of amber, hare, goose, caribou and musk ox bones. Ate at 1700 and debated going to Gilman but too late. Found tent ring 300 yards north of western house.

Thursday, July 10. Couldn't drop off to sleep last night until 0600, then slept until 1200. Woke up with headache, eyes puffy, bitten all over. Today I am going to walk until exhausted. Coffee and peanut butter then off for Gilman at 1700. Left sleeping bag and tent behind and took just ground cloth and fly. Walking along the lake shore a male and female ptarmigan ambled along about 10 feet ahead of me; an eiderduck waddled slowly to the shore and swam away. Two white Arctic hares danced along on their hind legs with their front paws clutched against their chests and Arctic char swam in the shallows opposite the gravel cliffs. There were musk ox bones and pieces of wood all along the shore. Much of the wood had been sawed, probably the work of Peary's men. Coal and amber on old sand beach about 100 feet above lake level. Game trail very pronounced along north shore—musk ox, wolf and fox prints in mud. Arrived at Gilman Delta 2200—tired but feeling good. Beautiful camp site—sand and gravel—ice up against the shore—clean water (water at Turnabout been muddy ever since the storm). Tried to sleep at 2400, couldn't. Finally got up and walked around the delta and up the valley. Saw four musk oxen across the river. Walked until 0500. Found several tent rings and caches on this side of the river.

Friday, July 11. Dropped off about 0600, slept till noon. No sleeping bag. Woke up a little stiff and chilly, quickly walked it off. I mapped the tent rings and scratched around them but found nothing. Everything, animal bones and pieces of wood, appeared to be on the surface. About 1700 I tried to cross the many channels of the Gilman. The ice water off the glacier was up to my waist, and half way across my legs became numb and I retreated. Low on food and tobacco here; so set off for Turnabout camp at 2000. Saw ptarmigan and hares on way home. Planned big dinner but when I arrived at 2315 had peanut butter and crackers while waiting for coffee to perk and immediately after turning off stove dropped off to sleep without drinking coffee.

Saturday, July 12. Planned to go back to Gilman today with more food and stay for a few days, but it looked like a storm back there. Six loons outside at the lake ice edge crying their lonesome call to the quiet sky. Amazing thing the cry of the loon when you are alone. Couldn't get to sleep by 0500 so I threw a rock at the loons to get them to fly away.

Sunday, July 13. Finally asleep by 0600. Slept until 1300. Lake water up considerably and very muddy. Got going about 1730. Beautiful walking along. Saw three hares and two ptarmigan. Was going to clobber the hen—dying for fresh meat—but couldn't bear to separate the happy couple. Little chilly, glad I brought my sleeping bag, hadn't planned to.

Monday, July 14. This morning as cold as any at Lonesome Creek. Put in a good twelve-hour archaeological day. Found a harpoon socket and tip of a harpoon point in F4 and a piece of a whale bone artifact, probably part of Dorset composite knife handle, in the midden at the point. The worst thing about being alone is that you repeat phrases of songs over and over. Repeated "what makes the lamb love Mary so, Mary so etc." a thousand times. The river will strike a note and hold it for hours. Strange how many tones there are in the rushing water of a river.

Tuesday, July 15. Very cold last night. Slept from 2300 to 2400, then couldn't get back to sleep. Walked around and troweled some more. Found a piece of whale bone in F4 and a crude antler fish spear, possibly Dorset, under a rock in F5. Worked steadily until 0100 then made two attempts to cross the river. On second attempt almost got swept out into the lake. Now 0430. Sun came out for first time in two days. Beautiful. Started to warm up. Believe I can sleep now until noon.

Friday, July 16. Slept until about 1500. Woke up with a splitting headache. Worked until 1800 then went back to sleep about 1900. Woke out of a sound sleep 2100. Tent gave a little lurch and noise of tin cans outside so I knew it was wolf or fox. Unzipped the tent and saw seven wolves all around me. I shouted and six ran away a little ways and stood watching. The seventh stayed about ten feet away staring at me. Throwing rocks didn't do much good; so I got out the Webley .455 and shot at the gravel near his feet. They then all took off up the valley. Two musk oxen had been grazing on a mesa up the valley and the wolves surrounded one but took off when the other musk ox charged them. This afternoon will probably head back for Turnabout.

Saturday, July 17. Never did get to sleep; so had a big breakfast and went out to work at 0800. Very cold. Finished up and left the Gilman at 1700. Pack very heavy, estimate fifty to sixty pounds. Shoulders awfully tired. Signs of wolf pack along shore but they hadn't bothered camp at Turnabout. Cold storm coming up as I hit camp. Shelter of tent felt good. Water just solid mud. Made some coffee anyway. The banging of the tent in the wind kept me awake until 0500 then slept until 1200.

Sunday, July 18. Woke up feeling pretty good. Waded out up to the knees to get some clean ice from big pile blown on rocks. Numbing cold but I was so thirsty. Bob and Barry were due back yesterday. The agreement was that if they were forty-eight hours overdue I was to walk back to base camp (twenty

miles and two rivers away) for help. I'll bet anything they went on to the weather station at Alert. If so, no telling when they will be back. Truly beautiful at midnight. Read, cataloged, and caught up on archaeological notes until 0700.

Monday, July 19. Finally dropped off to sleep about 0830 and woke up about 1400. Reasonably certain the boys have made it to Alert and sent a radio message back to base camp. Still too much ice along the shore to get a boat through. Hope I don't have to start the long trek back to base camp after 48 hours. I feel it would be unnecessary yet it really is the only thing to do.

About 1800 walked four miles east along the height of land to look for them and any possible sites. Returning to camp had a real windfall. Wind had blown ice crystals to beach right in front of camp, busily gathered, melted, then filled every receptacle with reasonably clean water.

Tuesday, July 20. Went to sleep about 0800, up at 1300. Nothing to do but wait. Will start for base camp at 72 hours overdue—midnight tomorrow night.

Wednesday, July 21. Went to sleep at 0900 until 1300. Now 1700 so shall finish up on west house and pack to set off at 2400. At 2130 here came the boys. They had gone on to Alert and thought I would have gone to the Ruggles River. They had almost camped out tonight, which would have been tragic.

Wednesday, July 23. Boys had spotted a tent ring about six miles inland so I walked in to look at it, returning 2130. Only a few pieces of cut antler. Pretty uneventful day. Very cold and rainy.

Thursday, July 24. Rested in morning. Packed up about midnight with fifty-pound pack (no tent or ground cloth) for exploring along south shore. Went down Salor Creek for a few miles, crossed it, and then across the height of land to a spot opposite where Greely had made his fourth camp. Made cold camp on damp sand beach.

Friday, July 25. Slept fitfully until 0900. Packed up and walked about nine miles. Saw two musk oxen

asleep until I walked right up to them. Found one poorly defined tent ring. Ground all silt, hummocky badlands underlain by fossil ice. When within sight of Ruggles River, turned back for the fifteen-mile hike to Turnabout.

Saturday, July 26th. Slept from 2200 to 1400. Now that the south shore is done, there is nothing to do but relax until the boat comes down from base camp.

Sunday, July 27. About 1700 Roger Deane and Ian McClaren showed up with two boats. They had found an open shore lead. Ian had caught some Arctic char and Roger had pipe tobacco. Both tasted wonderful. Struck camp and took off about 2100. The lead was open to the Gilman River but two miles above Section Creek we were stopped by ice and made camp. Soon after, a lead opened up again; so we set off only to be stopped by hard ice about eight miles from base camp.

Monday, July 28. All slept until 1500, then started hauling one of the boats across the ice by walking along shore in ice water up to our waists. Reminded us of some of the early British Arctic expeditions. Finally cached boat and walked the last two miles into camp, arriving about 0230.

Tuesday, July 29. Hauling party all too tired to sleep. Got up at 0800 for a good breakfast. Afterward I set off for Blister Creek to excavate the two small tent rings there—no artifacts. While I was digging I suddenly looked up and there was a fox looking at me from about five feet away. I talked to him and quietly reached for my camera but he trotted off.

Wednesday, July 30. Spent the day bringing notes and maps up-to-date. After supper decided to go down to John Tener's camp on Snow Owl river. While I was sitting there in the tent having coffee, a fox in summer pelage stuck his head in the tent. On the way back to camp I saw a twenty-foot exposure of fossil ice and two musk oxen.

Thursday, July 31. Beautiful day. First sun in couple of weeks—since July 19 I believe. Bob and I took off in the canoe for the Gilman Delta. Arrived about 0300. Caught a couple of Arctic char, fried and ate them and off to sleep.

Friday, August 1. Started excavating a small round house built of colossal rocks (some estimated at more than 1,000 pounds). House filled with windblown sand with floor at three feet below the surface. Found only one small chert scraper (probably Dorset). At 2300 Bob took off to meet Keith Arnold who is making his way down Section Creek. Wolf on terrace above my old camp. Geese flocking to go south. Willow leaves turning—winter coming.

Saturday, August 2. About noon Bob showed up with Michel Brochu, Keith, and two dogs—one with a paw nearly torn off in a fight and the other a bitch almost due to whelp. I took off in the canoe with Bob, Michel, and the two dogs. Decided to chance a trip to the Ruggles along the ice front. My cache was intact, even my favorite caribou skin left between two rocks. I started to excavate right away (0300) in case I had to leave suddenly.

Sunday, August 3. The next nine days became a blur of constant working, little sleep, and no real meals. The Thule house was a fascinating one with everything the man and woman occupants had owned still in place. After such a disappointing summer I was determined to excavate, leaving everything in place for mapping. Bob and Michel left, leaving me the two dogs. The poor injured one howled all night. Several times I nearly decided to shoot it to put it out of its misery.



9.2/ Knuth (R) and Maxwell at Independence II Solebakken site

Monday, August 4. About 1500 a musk ox on the opposite bank tried to cross the river. Got very angry, snorting and prancing. Would have made shambles out of camp. Worked steadily until 0100. Ate a little out of a large can of roast beef.

Tuesday, August 5. Worked steadily with little time out for eating or sleeping. Musk ox tried to get across the river again.

Wednesday, August 6. Awakened at 0600 by airplane landing. Rushed out in sock feet. It was Terry Moore, flying-president of the University of Alaska in a Piper Super Cub on floats. He flew me back to base camp for mail and breakfast, then back to Ruggles. Musk ox tried to cross river again. Shot Webley into air, which scared him off. At 2300, Dr. Moore picked me up again and flew me around the lake at low altitude looking for more sites.

Thursday, August 7. Up at 1100, worked frantically with no meals, two cups of coffee, until 2400, many more artifacts. Dr. Moore dropped me a message at 2200 said icebreaker Atka in Chandler Fiord late tomorrow.

Friday, August 8. Working against time. Haven't stopped for full meal or night's sleep since arriving here. Roger dropped in by boat for supper. He is stranded at the east end of the lake. Shared my can of roast beef with him. Good to have company.

Friday, August 9. Awakened at 0800 by terrific racket of helicopters passing over. One, Charley Le Boeuf, stopped in to pass the time of day. Piaseckis and Bell helicopters back and forth all day ferrying fuel and supplies. Dingle, Hal Sandstorm, and Keith Arnold and dogs dropped off at my camp. Boys amazed at how cold it is here compared to elsewhere around the lake.

Sunday, August 11. Awakened by Charley Le Boeuf at 0800 who wanted to know when I wanted to be picked up. Later in day Bell landed twice, Piasecki once to pick up injured dog. Helicopters over all day taking pictures

of musk oxen, Terry Moore over from Eureka, boat on lake droning away with echo sounder. Navy P2V ice recon plane over—all we needed was a hot dog stand. Will be picked up tomorrow afternoon. Boy I'll be glad to get warm.

Sunday, August 11. Up early and rushed to finish shining.

This essentially ended my research for the summer. I spent the next two days in base camp, cataloging artifacts, resting, and eating. On August 13 Charley flew me up to a group of tent rings east of Snow River, but I found nothing. On the 14th, he picked me up and deposited me on the USCG *Atka* ice breaker. Took a great hot shower, drank beer, and smoked cigarettes. Traveling across Kennedy Channel on the *Atka* we received word that Count Eigil Knuth, the well-known Danish archaeologist, excavating a very important site on Polaris Promontory, Greenland, was running behind schedule. He requested that I come over to help him finish up (fig. 9.2). We worked together for four days on a very interesting Independence site, then I back on the *Atka* on the 18th (fig. 9.3). By then I was through with the wilderness life for the year. I arrived on Thule Air Base August 20 and with a series of planes and trains arrived home on the 24th.

Epilogue

The summer had many enjoyable moments and some difficult ones. I had spent forty-two days completely alone, an experience I do not choose to repeat. According to the map, I had backpacked some 736 kilometers, a feat I never repeated. Scientific results of the survey were not as impressive as I could wish, nor as it turned out, as definitive as if I had helicopter support throughout the summer. The results are best summed up by a few sentences from the published report:



9.3/ Maxwell and Knuth examine artifacts on board *Atka*.

Eskimos from an as-yet unexcavated, or unidentified, settlement made a limited, seasonal use of the region in the period comparable to that covered by the stratified Comer's Midden (northern Greenland) from the transition of Thule to Inugsuk culture (and) through the middle part of the Inugsuk development. . . . In the light of previous theories we considered it a foregone conclusion that evidence of early migrations lay in this little explored region. . . . However, we found no evidence of such migrations and in light of the region covered by the survey consider it unlikely that these movements took place through the region. Rather, the Lake Hazen valley appears to have been a cultural cul-de-sac, used as a seasonal hunting ground by people marginal to pervasive settlements on Greenland. These hunting trips stopped in the mid-fifteenth century for unknown reasons. (Maxwell 1960:88)

These conclusions would have been quite different had I only walked the same distance west of Lake Hazen as I had walked east. Twenty-two years later, Patricia Sutherland (1980) found that there were a number of Independence sites between the west end of the lake and the western fiords of Ellesmere. It would appear that Steensby (1910) had been right and that this was an early migration route to Greenland.

Demography and Interaction: An Appraisal of the Core Area Concept in Paleoeskimo Studies

DANIEL ODESS

The 1993 Elders Conference offered a rare, possibly unique, opportunity for the younger members of the arctic archaeological community to sit at the feet of their intellectual forebears and learn firsthand about the history of their field of study. In keeping with the historical theme of the meeting, I have chosen to revisit the “core area” concept—an idea that has structured many researchers’ writings about Paleo- eskimo demography and cultural development in the Eastern Arctic.

Core Area Concept and the Question of Occupational Continuity

The core area concept arose in the late 1960s and early 1970s (McGhee 1972a), but appears to have attained its current form at the 1973 School of American Research Seminar Series (SAR) meeting in Santa Fe, New Mexico (Maxwell 1976b; McGhee 1976b). At that time, several of the participants saw the ecologically rich area surrounding Foxe Basin as a region continuously occupied for what was then thought to be the 3,000 years of Eastern Arctic Paleoeskimo prehistory. At the time of that meeting, Igloodik was the only location where there was known to be an uninterrupted sequence of occupation from Sarqaq (Pre-Dorset) until the Late Dorset period (Meldgaard 1960b, 1962). While various scholars had conducted research in other areas (most notably McGhee at Port Refuge and Bloody Falls; Maxwell in the Lake Harbour area; Taylor in Ungava and on Victoria Island; Collins on Southampton Island and in Frobisher Bay; Harp in



Newfoundland, eastern Hudson Bay, and in the Thelon River drainage; Knuth in Peary Land and on Ellesmere; Mary-Rousselière on North Baffin; and Fitzhugh and Tuck in Labrador), their impressions based on this early work were that the Paleoeskimo occupations of these regions were punctuated by periods of abandonment (McGhee 1976b). Following this abandonment, these areas were eventually recolonized by people whose technological inventory shared many stylistic similarities with contemporary people in the core area. The late Father Mary-Rousselière appears to have been one of the strongest proponents of the core area concept at the SAR seminar. He concluded his discussion of the Paleoeskimo prehistory of northern Baffinland with the following:

It is tempting to see the Foxe Basin region in Dorset times, at least during certain periods, as exerting a strong attraction on populations, and at the same time radiating its cultural influence not only in the technical domain of weapons and tools, but also in that of clothing, fashion, art, and ideology, as was the case in modern times. (Mary-Rousselière 1976:57)

Research since the SAR meeting has tended to confirm the impression that some areas outside of the core

were periodically abandoned, but it has also placed this initial impression on increasingly thin ice in other areas. For example, Banks Island is at the westernmost end of the region known to have been occupied by Dorset people. In core area concept terms, it is very peripheral. Work by Arnold (1980) and Le Blanc (1994) indicates that Paleoeskimo people occupied the area during the period from 2800 to 2300 years ago. The lithic tools they recovered—notched end and side blades and ground burin-like implements—are not greatly different from those used by contemporary Groswater (transitional from Pre-Dorset to Dorset) peoples in Labrador and elsewhere, though these forms seem to persist in the Banks Island-Cape Bathurst area for a few hundred years after they are superceded by “Early Dorset” forms (Cox 1978; Tuck and Fitzhugh 1986:165) farther east. Arnold attributes this stylistic lag to a period of isolation from contact with the East following the initial colonization.

The peripheral nature of the area is further highlighted by examination of the organic portion of the assemblage. At both the Lagoon (Arnold 1980) and Crane sites (Le Blanc 1994), harpoons with lashing slots and needles with rounded heads and circular eyes appear to be more at home in Alaska’s contemporary Norton tradition. The intermediate geographic location of sites in the Banks Island-Cape Bathurst area between Alaska and the core area combines with their technological inventory to suggest that contact between the two conceptually discrete cultures (Dorset and Norton) has produced something of a cultural hybrid, which Le Blanc argues should be termed the Lagoon Complex. Following this hybridization, it appears that the area was eventually abandoned. The ultimate impact of Norton-tradition ideas and artifacts on Dorset culture (and vice versa) remains unclear.

The findings from the Banks Island-Cape Bathurst area can be seen to support the core area hypothesis in two ways. First, it is a geographically and perhaps ecologically marginal area that was colonized and

eventually abandoned. Second, technological similarities between those who lived there and people in the core area were greatest at the time of colonization and can be seen to have diverged over time. After 500 years of occupation, the material inventory of people in the periphery appears to have been stylistically and technologically out of step with developments elsewhere in the Dorset homeland, a pattern that may, in fact, foreshadow the demise of this and other populations in the periphery.

While the predictions of the core area hypothesis have been affirmed by the results of post-SAR research in the vicinity of Banks Island, work in Labrador since 1973 has tended to challenge the validity of the model, at least in its unmodified form. Labrador has been the site of an intensive research effort, which began prior to the SAR meeting (Fitzhugh 1972b, 1976b; Tuck 1975b, 1976b; Tuck and Fitzhugh 1986). Since that time, continued work by members of the Torngat Archaeological Project has documented a more or less continuous occupation for the area north of Nain from the early Paleoeskimo period until ca. 650 B.P. (Cox 1978, 1988; Fitzhugh 1980a, 1980b; Jordan 1980). However, Fitzhugh notes that radiocarbon dates are lacking from the time between 1400 and 1000 B.P., suggesting the possibility that the area was abandoned during that time. This situation has parallels in the core area; Maxwell (1985:216) notes a lack of sites dated between A.D. 200 and 500 in all areas of the core except the head of Foxe Basin and the north Baffin coast.

Paleoeskimo peoples appear to have colonized the area south of Nain later than they did the northern Labrador coast. It appears that Paleoeskimo people began pushing south along the coast a few centuries after 3000 B.P., since it is at that time that Groswater Dorset people occupied the Buxhall (Fitzhugh 1976b) and Postville (Loring and Cox 1986) sites, as well as various sites on the Quebec North Shore (Pintal 1994). It is also during this time that Paleoeskimo people first reached Newfoundland. Evidence for their presence is

seen in the extensive Groswater Dorset occupations at Philips Garden East at Port au Choix (Renouf 1991, 1994) and Factory Cove (Auger 1986), and by the first appearance of Newfoundland cherts in Labrador assemblages at this time (Steven Cox, personal communication 1990). The archaeology of Labrador does not conform to the patterns predicted by the core area hypothesis because the expected cycles of colonization and abandonment either do not occur or do so only once. In addition, there does not appear to be a distinct Labrador-style of Dorset, a situation which probably reflects continued interaction between people there and those in the core.

It is not my goal here to provide an update of demographic patterns in all the areas where Paleo-eskimo research has been conducted since the SAR meeting. Rather, it is to show that the perception of Igloodik as the only area continuously occupied throughout the Paleo-eskimo period was in part an artifact of the uneven distribution of research conducted prior to 1973. As a result of research following the SAR meeting, it has become clear that the demographic patterns in some areas are in close accord with those predicted by the core area hypothesis, while those in other areas clearly are not, a situation that, as Cox (1978:115) notes, suggests the existence of multiple core areas or problems with the concept entirely.

In order to assess the continued utility of the core area concept in Eastern Arctic archaeology, it is first necessary to examine those features of the archaeological record that it explains and the explanatory mechanisms it invokes. To my mind, the core area concept attempts to explain two distinct but interrelated elements of Paleo-eskimo prehistory. These are: (1) demographic changes over space and time, and (2) the perceived homogeneity of tool styles seen throughout the Dorset homeland at any given time.¹ In general, these are ultimately viewed as the cultural consequences of local or regional fluctuations in the productivity and predictability of the arctic ecosystem.

The Core Area Concept and Paleo-eskimo Demography

The perception that peripheral parts of the Dorset area were periodically abandoned and recolonized was one of the things that originally led to the promulgation of the core area hypothesis. These peripheral areas are perceived to be ecologically marginal, at least from the perspective of human adaptation (e.g., Fitzhugh 1973, 1976a). Resources in them are less abundant or predictable and more prone to fluctuation than those found in the core area, with the result that people living in the periphery either starved to death or abandoned their homes and retreated back to the stability of the core area in search of food. When, after a few years or several generations, resources again became sufficiently stable and abundant, people recolonized the periphery, presumably because of population pressures within the areas still occupied. Thus, the first part of the core area hypothesis is an attempt to explain the spatial-temporal elements of human population dynamics in the context of adaptive responses to a harsh and fluctuating environment. As noted above, these demographic patterns are seen as cultural consequences of regional variation in Eastern Arctic ecological productivity and predictability (Fitzhugh 1973).

If these changes in paleodemography are ultimately tied to perturbations in the physical environment, it should be possible to correlate them with events and trends recorded in the paleoclimatological record. Several researchers have attempted such correlations (e.g., Fitzhugh 1973, 1976a; Maxwell 1985; McGhee 1972a), but the demonstration of a concrete link has proven an elusive goal. Three factors combine to muddy these waters and cast the ship of inquiry adrift in an ice-choked sea of speculation. In brief, these are an uncertainty about the temporal relationship between climatic and demographic events arising largely from the imprecision of the radiocarbon technique; a poor understanding of the spatial scale at which such events operate; and, most important, continued uncertainty about the relationship between the paleoclimatological

trends interpreted from proxy data and the status of resources on which people were dependent. One need only look to the ongoing debates about the implications of a projected increase in mean annual temperature associated with global warming to see that the “on the ground” impact of prehistoric temperature fluctuations recorded in deep ocean cores and glacial ice is poorly understood. Until such time as we can accurately model the effects of both short- and long-term climatic changes on such key variables as the stability and migration of animal populations; frequency, intensity, and direction of storms; sea-ice formation, etc., attempts to causally link specific climatic and demographic events will remain as speculative as they are intuitively appealing.

Leaving aside the problems inherent in trying to relate demographic trends to climatic changes, any discussion of population dynamics within a specific locale or region as a whole requires that we be able to date occupations, or their absence, in either relative or absolute terms. The vagaries of dating in the Arctic have long been known (e.g., Arundale 1981; McGhee and Tuck 1976; Morrison 1989), and this is not the place to launch into a new discussion of techniques or calibration curves. However, recent work on calibrating dates to facilitate comparisons in Alaska (Gerlach and Mason 1992; Mills 1994) is leading to a partial rethinking of culture chronology and the temporal significance of artifact styles in the Western Arctic. Similar problems exist in the Eastern Arctic, with researchers reporting dates from a variety of materials run by a variety of radiocarbon labs using several different assaying and calibration techniques. As a result, it is difficult to compare different researchers’ dates for the occurrence of a particular phase of the Arctic Small Tool tradition with even the crude temporal confidence inherent in the radiocarbon technique. The problem is further compounded by the fact that artifacts are frequently used as index fossils, with the result that style and time period have come to be equated with one another.

As noted above, the core area hypothesis postulates that the range of Dorset as a culture expanded and contracted in response to environmental stimuli, with people abandoning the periphery for the economic security of the core area in times of resource stress. From a logical standpoint, it is difficult to envision how this would be accomplished, since accepting groups of half-starved strangers into their territories would put the occupants of the core area near or beyond the carrying capacity of their environment. Given the existence in hunter-gatherer societies of mechanisms such as post-partem sex taboos and infanticide, which are directed at sustaining population levels below the carrying capacity of the environment, it seems unlikely that people in the core area would accept large numbers of what are essentially economic refugees, since doing so would put them at risk. Elsewhere in the Arctic where population movements to escape starvation have been documented (e.g., Burch 1980), they have generally involved numbers of people that are small, and geographic areas much more limited than those being modeled in the core area hypothesis. I do not question that perturbations in the physical environment caused starvation or forced migrations in prehistory, but I would simply point out that the idea of people withdrawing in large numbers into the core area conflicts with what we know of hunter-gatherer behavior from the ethnographic record.

To my mind, one of the strongest criticisms of the core area concept is that it relates all episodes of abandonment to changes in the physical environment. As Rowley (1985) has shown, mobility was an important factor in allowing Inuit during the historic period to escape environmental and social stress. Her findings suggest that there were long-distance migrations of relatively large numbers of people, migrations that I maintain could cause an area to appear abandoned. In contrast to the model proposed by the core area hypothesis, however, Rowley indicates that in over half of the incidents where a cause for the migration was known, it was factors in the *social* rather than the

physical environment that motivated people to move. I see no reason to think that the desire to escape social tensions did not cause similar movements during the Paleoeskimo period, and suggest that the difficulty in relating some episodes of abandonment seen in the archaeological record to variations in the physical environment may in some cases relate to their social origins rather than to the concerns raised above.

Core Area Concept and the Perception of Interaction

The second aspect of the archaeological record that the core area concept has been used to explain is the perceived stylistic homogeneity of contemporary artifacts from sites all over the Eastern Arctic. In contrast to the biogeographical elements of demography discussed above, this is a cultural phenomenon, as artifacts are held to reflect the ideas of their makers. To the extent that they are judged similar, these artifacts are taken as proxy data that represent the shared nature of ideas held by their makers in different regions of the Dorset homeland. Paleoeskimo tool kits generally exhibit a high degree of stylistic and technological conservatism, and the similarities between contemporaneous artifacts from widely separated sites are striking. When new artifact forms such as “tip-fluted” triangular endblades appear, they are thought to show up almost simultaneously in sites as widely separated as Philips Garden in western Newfoundland (Harp 1964a) and T-1 on Southampton Island (Collins 1956b), a phenomenon that led one elder to remark that:

Throughout both Pre-Dorset and Dorset periods, there appears to have been a regularity of interaction among these geographically distinct groups. This is marked by exchange of technological information to the degree that minor discrete style differences on artifacts appear to emerge almost simultaneously throughout the core area. (Maxwell 1985:82)

Thus, the core area concept is also used to explain the movement of ideas and information. In essence, the core is seen to act as a central clearinghouse, serving

to disseminate the latest information on, among other things, harpoon-socket styles, tip-fluting techniques, and burin-like tool hafting protocols. There is an assumption that cultural developments moved from core to periphery, not the other way around, and not between peripheral regions.²

Recently, Sutherland (1992) has challenged the idea of homogeneity, and suggested that early Paleoeskimo assemblages show greater variation than previously recognized. In an argument similar to that raised by Arnold (1980), she attributes this variability to the long-term occupation and relative isolation of northern Ellesmere Island. Unlike Arnold, however, she sees the process of adaptation to local resources as driving the changes in tool morphology. At issue here is the question of how we measure variation, since deciding two artifacts exhibit significant stylistic or technomorphological similarity or dissimilarity entails making subjective judgments. Such judgments have strong implications for how we interpret the archaeological record because concluding that contemporaneous artifacts found in different areas are similar implies, at least in core area concept terms, some unspecified form of interaction between the peoples in question. The corollary of this “if then” hypothesis is that dissimilarity is equated with lack of interaction (Odess 1998).

The relationship between formal similarity and interaction should be phrased as a hypothesis, and can be tested independently using archaeological data that do not entail subjective or even objective judgments of style. The following example should clarify how such an approach can aid in interpretation of the archaeological record. Recent work at Willows Island 4 (Odess 1996, 1998), a site located on a small island in the outer part of Frobisher Bay, indicates that Dorset people in that part of south Baffinland continued to make and use Tyara Sliced and Dorset Parallel Sliced harpoons for at least 400 years after the 300 B.C. date when they are thought to have gone out of style elsewhere (Maxwell 1985:197). Using only the stylistic criteria

normally employed, one would likely argue that the Frobisher Bay Dorset were a remnant population cut off from interaction with people elsewhere in the Eastern Arctic and caught in the isolation of a cultural backwater. Such an idea seems untenable, however, in light of other archaeological data. Exotic materials, such as Ramah chert from Labrador and a distinctive banded-brown chert thought to come from the Southampton Island area, continue to appear in the Willows Island assemblages from this time period, indicating that the site's occupants maintained contact with their countrymen elsewhere in the Dorset homeland while at the same time making artifacts that were distinct from those made by the people with whom they were in contact (Odess 1998, n.d.).

Willows Island is on the margin of the core area as illustrated by Maxwell (1985:81). The continued persistence of the sliced harpoon forms suggests that the cultural influence of the core, at least in terms of style and technology, was less than previously thought. In light of the continued persistence of these harpoon forms alongside contact with those who had abandoned their use, we are confronted by questions about the meaning of style in prehistory. The core area hypothesis has assumed style to be a reliable temporal indicator across regions, an idea that conflicts with the example given above.

More than any other class of artifact found in arctic assemblages, harpoon heads have been treated as index fossils that are thought to indicate their period of manufacture. Indeed, changes in harpoon head form are *the* indicator of the transition from Early to Middle to Late Dorset (Maxwell 1985:198). Given the persistence of "sliced" (Early Dorset) harpoon forms well into the period generally considered to be Middle Dorset and the evidence against treating the occupants of Willows Island as an isolated population, it is clear that our assumptions about the temporal sensitivity of artifact styles need to be reexamined. In light of the data from Willows Island, it is tempting to suggest that the Frobisher Bay Dorset were a culturally conservative

group who were aware of new technology but saw no reason to abandon the old. Alternatively, one might argue that at least during the time period in question, harpoon heads were meaningfully constituted objects of material culture. Following this line of reasoning, people in Frobisher Bay used them consciously to signify membership in a quasi-ethnic group distinct from their close-socketed harpoon using neighbors in the Central Arctic, as well as to hunt animals. Similar ideas about the dual functions of material culture have been put forth in general terms by Hodder (1986), and at least hinted at by Gerlach and Mason (1992) for the Western Arctic. Whether or not harpoon heads at Willows Island 4 served these dual functions is a question to be answered by future research. In any case, if work on Paleoeskimo demography is to continue in a productive fashion, it will require firmly dating occupations of individual site components independent of artifact style.

Conclusions

From the above, it might appear that I think the core area concept to be so fraught with inherent problems and so superseded by research results since it was first articulated that it has little or no utility for contemporary students of arctic archaeology. This is not the case. The core area concept represented a significant step forward from purely culture-historical concerns to provide some of the first explanations of the regional-level patterns observed in the archaeological record and their linkages with ecological concepts of stability and instability. While the passing of time, with its attendant theoretical and methodological developments and ever-growing midden of data, has highlighted some of the weaknesses in its original formulation, the core area concept remains useful in part because it is readily modified in light of new developments. For those of us just embarking on careers in arctic research, it continues to be a model of demography and interaction against which we can test our own findings. Dissonance between our results

and those predicted by the model serves to highlight significant elements of our data and to point us in useful directions for future research.

Acknowledgments

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Notes

1. See Odess n.d. for discussion.
2. See Cox 1978 for a discussion of interaction between regions outside of the core.

The Early History of the Cape Dorset Culture

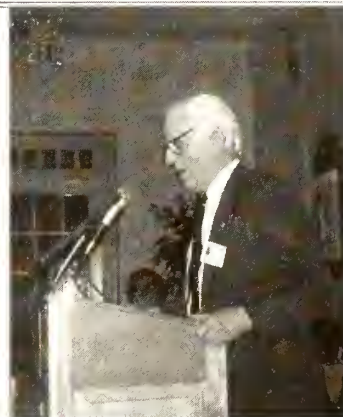
GRAHAM ROWLEY

In describing the discovery and early history of the Dorset culture, I thought I would take an autobiographical approach because this would tell you what one Eastern Arctic archaeologist used to do when he was young—and that is what Elders are for.

My story begins in Cambridge in June 1935. I had graduated in natural sciences and had then spent two years pursuing what the university described as “diligent study” in archaeology. I had no idea of how to earn a living. One morning a man called Tom Manning came to see me. I did not know him but he brought a letter from Louis Clarke, the eccentric and respected curator of the University Museum of Archaeology and Ethnology, who brought style and unpredictability to the museum’s activities. His letter was short: “My dear Rowley, This is to introduce Mr. Manning with whom, I hope, you will go to the Arctic. He will explain things to you. Yours sincerely, Louis Clarke.”

Tom told me he was planning a small expedition to the Canadian Arctic, to start in the spring of 1936, and he asked if I would join it as the archaeologist. As nobody else had said they wanted an archaeologist, I found myself a member of the British Canadian Arctic Expedition. The others were Reynold Bray as ornithologist, Pat Baird as geologist, and Peter Bennett as surveyor. We were all in our early twenties, single, and had been up at Cambridge, except Reynold, who was married and had been sent down from Oxford.

Tom’s plan was to explore the east coast of Foxe Basin, then the least known part of the Arctic. Most of



the west coast of Baffin Island was shown on maps by a dotted line because no white man had been there. Bernhard Hantzsch, a German zoologist, had done the most to map this coast, but he had died there in 1911 from trichinosis through eating raw polar bear meat. The Danish Fifth Thule Expedition had planned to complete the map of Baffin Island. In 1923, Peter Freuchen tried, became separated from the Inuit he was traveling with, lost his way, and froze a foot. The archaeologist, Therkel Mathiassen, tried later the same year, but an outbreak of dog disease stopped him.

The Eastern Arctic in 1935 was very different from today. It was the home of about 5,000 Inuit, with a few Hudson’s Bay Company trading posts to serve them. At some of the posts there was a Royal Canadian Mounted Police detachment and perhaps a Roman Catholic or Anglican mission. The only contact with the south was a ship that called at each post once a year and stayed a day. She brought in supplies and mail and took away the fur. There were no aircraft. Most posts had a radio receiver but not a transmitter. There were no schools. The only medical services were

two small hospitals, and the only way to reach them was by dog sledge in winter or by very small boat in summer. The total white population was little more than fifty. Very few Inuit lived at the posts. Most survived by hunting and trapping from small camps along the coasts. Away from the posts, one had to depend on oneself or on the very dependable Inuit.

Tom's plan was to sail in a 30-foot whaleboat from Churchill to Southampton Island, to winter at Repulse Bay, from there to cross Foxe Channel to Baffin Island, and then to sail north along the unknown coast. We were to set out early in 1936 and to be away for two or three years.

We did not have much money. The Royal Geographical Society lent us surveying instruments and awarded us a small grant. We were given a few other grants, none of them large, and generous manufacturers provided us with Chivers jam, Cadbury chocolate, Barneys tobacco, and Haig whisky. The Canadian Government allowed us to import our food and equipment without paying duty. Our grants and the value of what we were given totaled about \$600, which of course went further than it would today, but nothing like far enough. The rest we had to make up ourselves; we were not rich, so we had to do everything as cheaply as we could.

I read all I could find about the area and its archaeology. By far the most important book to me was Mathiassen's report describing the Thule culture he had found in the old stone houses so common in the Eastern Arctic, which he thought had been built by the Tunit of Eskimo tradition. I also saw Diamond Jenness's short article in the *Geographical Review*, suggesting that there was more to Eastern Arctic archaeology than just the Thule culture. I had time for a quick visit to Copenhagen to meet Therkel Mathiassen, Kaj Birket-Smith, and Helge Larsen, and to see what the Fifth Thule Expedition had collected.

We sailed from England in March 1936 and traveled first to Ottawa where I met Diamond Jenness. He helped me in every possible way, and showed me the

arctic archaeological collections in the National Museum, pointing out how some of the artifacts were quite different from Mathiassen's Thule types. They looked older and never had drilled holes, which were very common on the artifacts that Mathiassen had excavated. Jenness believed they must belong to a different and earlier culture, which did not know about the bow-drill, and which he had called after Cape Dorset because a collection from there included many of these strange artifacts. Mathiassen and several other archaeologists did not agree that there was a Dorset culture distinct from the Thule. Mathiassen considered it a peculiar, very locally stamped phase of the Thule, and thought there was no archaeological evidence for a pre-Thule culture in the Eastern Arctic. When I asked Jenness what an archaeologist could most usefully do in the north, he said the Dorset culture would not be fully accepted until a site was found that had only Dorset material. Jenness also introduced me to the archaeological and arctic fraternities in Ottawa, some of whom the other elders may remember: Erling Porsild, W. J. Wintemberg, Percy Taverner, Rudolph Anderson, Slim Monture, John Cox, Douglas Leechman, and Kenneth Chipman.

We left Churchill early in June in a thirty-foot whaleboat that we called the *Polecat*, and sailed to Bay of Gods Mercy on Southampton Island. Here there were some old stone and whale bone houses for me to excavate, but it proved to be a fairly recent site where the Sadlermiut had lived, probably until they died early this century from an epidemic. There was a single Dorset harpoon head, presumably collected by some Sadlermiut archaeologist. Clouds of mosquitoes made excavation miserable. They covered the old houses like a blanket. We smoked as much as we could, rolling our own cigarettes, but my hands were always greasy with ancient Sadlermiut blubber, and old blubber tastes bad and smokes even worse. For the arctic archaeologist the greatest technological advance in the last half century has been the development of effective fly repellents.

We then sailed to Walrus Island to kill a walrus for dog food. I found some old houses there, which I started to excavate, but my work was cut short when our boat was caught on a lee shore, swamped, and nearly wrecked. We were able to repair the *Polecat*, but could not start her engine. The houses I excavated, when not drying our equipment or repairing the *Polecat*, yielded



11.1/ Abverdjar ca. 1934

both Dorset and Thule material, but there were only four or five possible camping places on this small rocky island. The same sites and the same stones had been used again and again and any stratigraphy had been destroyed. We had to leave when a favorable wind allowed us to sail to the Hudson's Bay Company's post at Coral Harbour where we were to meet the *Nascopie*, the annual supply ship. While waiting for her, I spent two or three days digging in some old houses at Kudluktok, a few miles west of the post, but there was no Dorset material.

After the *Nascopie* had come and gone, I went to Coats Island with a party of Inuit. Unfortunately, we all caught the "ship's cold," an annual event, and some of us were very sick. As a result, we did not reach the north of the island where I knew some Dorset material had been found. The few sites I could reach were all very recent. After returning to the Hudson's Bay Company post, Pat Baird and I crossed Southampton Island by sledge to rejoin the *Polecat*, sail across Frozen Strait, and walk to Repulse Bay.

At Repulse, there was very little dog food. Tom decided that Reynold Bray and I would have to sledge to Igloolik, where there was always plenty of walrus, and spend the winter there. We could not start until Christmas when there would be enough snow for

traveling. In the meantime, Reynold and I had to learn how to drive dogs, build snow houses, and other necessary skills.

We left Repulse Bay on December 21, passed where fourteen years earlier to the day Peter Freuchen had lost his way, and then his foot, before eventually reaching Igloolik. Here, a solitary Roman Catholic priest, Father Bazin, was living with the Inuit. The closest trading post and the nearest building that would not melt in the spring were three hundred miles away.

When Father Bazin heard I was an archaeologist, he told me about some artifacts the Inuit had found when digging turf for their autumn houses on Abverdjar, a nearby island (fig. 11.1). They looked different from the artifacts that were found in the old houses at Igloolik Point, and he had made a collection of them. He said I could have them if I went to Abverdjar where he had left them. Next day I sledged there and brought them to Igloolik. There were several hundred artifacts of chert, ivory, bone, and antler, and a single small knife blade of iron, probably meteoric as it had not rusted away; none were Thule, and they all appeared to me to be either known or likely Dorset types.

Reynold and I had learned a lot during our journey to Igloolik. We had learned we could travel with dogs on our own in midwinter. We had also learned it was

more efficient, more comfortable, and much more interesting to travel with Inuit. We decided we would not return to Repulse Bay but would try to complete the coastline of Baffin Island by traveling with two Inuit to the east. I won't describe this journey. We were lucky and managed to reach the point where Hantzsch, coming from the south, had to turn back. We also found a great peninsula and a large low island lying some miles off the coast.

I then made my way to Pond Inlet and on to Arctic Bay, where I learned at the post that there were several groups of old houses that I could reach to excavate that summer. I dug at four places, but everything I found was fairly recent. At one site I even dug up a rusty umbrella frame in what appeared to be the oldest part and where I had hoped to find some indication of the Dorset culture. That summer the *Nascopeie* took me south.

In Ottawa, I showed Jenness the collection Father Bazin had given me. He confirmed that it was all Dorset culture with some types that were new to him.

Back in Cambridge, I worked on the material I had collected, but I realized I had made a great mistake in not returning to Igloolik to excavate the site at Abverdjar. That summer I suddenly made up my mind, went to my bank, bought a ticket, and was back in Canada within a week. My Scientist and Explorers License and other permits were still valid, and I reached Churchill in time to sail in the annual supply schooner to Repulse Bay. Mathiassen's Naujan site was close to Repulse, but it was too wet and too late in the season to excavate. I was surprised to find some pieces of disintegrating native copper in the houses that Mathiassen had partially excavated but had subsequently been disturbed. The only serious digging I did there was for my spade, which I had carelessly left on the ground before an unexpected and heavy snowfall. It took me three days to find it.

I now had to get by sledge to Igloolik, three hundred miles to the north. An Inuk I knew agreed to take me as soon as we could travel by sledge. Most of our

load had to be dog food. We also had to carry everything I would need for a year. Nothing I lost, broke, or used could be replaced.

We reached Igloolik early in February. I would not be able to excavate until the middle of June at the earliest, and this gave me time for some exploration. I had, of course, read Boas's Smithsonian report on the Central Eskimo. It included a map showing routes that the Inuit told him were used for travel. All were well known except one that ran from Foxe Basin across the mountains of Baffin Island to Analaurealing, a fiord in Baffin Bay now called Cambridge Gulf. The Inuit at Igloolik told me that they did not know of anybody who had ever followed this route, and I decided to spend the next two or three months trying to find if it existed or if Boas had been misinformed. I would also discover what the country was like, as no white man had ever been there and only a few Inuit caribou hunters had been inland where the route was shown.

I set out with two men and a boy. We managed to find a way across Baffin Island as marked on Boas's map, though some parts were rather difficult, particularly the steep descent to the east coast. Here we had to throw all we had over a precipice before traversing down a very steep, snowy slope with the sledges turned upside down. Then we went to Pond Inlet and to Arctic Bay and from there back to Igloolik. On the way we often stopped at Inuit camps. Waking up one morning in a camp between Analaurealing and Pond Inlet, I saw that I was not in a snow house as I had thought. A stone supporting the roof was visible where the canvas lining of the house was incomplete. Close inspection showed that it was a stone house, and I felt I was in the Thule culture. I began to doubt Mathiassen's conclusion that the present Inuit represented a different culture, and were not descended from the Thule.

It was still too early to dig so I made another journey, this time to an island to the east, which Mathiassen had seen and named Koch Island but had not been able to visit. From the highest point of Koch Island I

had a very good view to the south and southwest of a much larger island not shown on any map. I could not cross to it because open water lay between it and me.

I returned to Igloodik and went on to Abverdjar. Most of the snow had melted, and I could see a gradual slope from which the Inuit had been cutting turf for their nearby autumn houses. There was little sign of occupation where the slope was undisturbed—just a few faint circular hollows in the turf, three or four yards in diameter. Up until then it had been assumed that the Dorset people, like the Thule, had lived in stone houses. With the help of two young Inuit, I spent July and August excavating, finding more than a thousand artifacts, including two of native copper; none of them were Thule. They all lay in the lower part of a thin layer of soil under the turf or on the underlying sand. There was no indication of walls, and some flat stones we found on the sand seemed to form partial pavements, but they did not appear to be related to the faint hollows. We had minimal equipment—knives, a shovel, and a tape measure, but I could lay out a base line and measure from it the position and depth of each artifact. We could also determine heights above sea level. We had no trouble netting enough fish and shooting enough game to feed ourselves.

There can be few archaeological pleasures greater than excavating a rich late Dorset site. Something can emerge from the thawing ground at any time and at any place and may prove to be a particularly beautiful carving, an exciting harpoon head, or something completely unexpected. I remember a piece of antler carved with a number of faces, more of which became visible as the soil around it slowly thawed. Eventually, there were about thirty faces, so expressive we could almost imagine them talking to us. We put unworked bones in a pile, and experienced hunters had no difficulty in identifying them. They represented walrus, ring and bearded seals, polar bears, caribou, hares, and very many foxes, but no large whales, narwhal, white whale, or musk ox.

Only unusually poor weather detracted from an idyllic summer. We had a succession of Inuit visitors, first by sledge and then by boat. They said they had come for a cup of tea. I think their real purpose was to make sure that nothing had happened to us.

Early in September I returned to Igloodik Island because a ship was expected to bring supplies to the Mission. This gave me an opportunity to spend a few days excavating at Arnacotsiaq, where there were some Thule houses. The ground was already beginning to freeze, but I found these houses had been built where there had been an earlier Dorset camp.

In mid-September, the ship arrived bringing everything to establish a Hudson's Bay Company post at Igloodik. She also brought the news that war had broken out two weeks earlier. I sailed south with her, reaching Montreal a month later. In Ottawa and with Diamond Jenness's very considerable help, I wrote a brief account for the *American Anthropologist* of the Cape Dorset culture and the site I had excavated at Abverdjar (Rowley 1940), before joining the Canadian Army and sailing for Europe.

This was how one archaeologist spent his time in the years just before the Second World War. Conditions are very different now. On an expedition, archaeology was only one of several studies and an archaeologist only one of several scientists. Often the needs of the others would have priority. To spend the short summer excavating, one had to live in the North for a whole year and one had to do something else as well as archaeology—in my case it was mapping and geography. Mathiassen had written reports on geology and material culture. Jenness had carried out studies of linguistics, anthropology, and folklore. Unless one was excavating near a post, supplies and equipment were limited to what could be carried, along with dog food, on a sledge journey, which in my case was more than three hundred miles. Food was anything that was available locally—in my case mainly walrus. Biting flies were a torment, and at their worst on days that were otherwise ideal for digging.

There were also great advantages. I lived with the Inuit and no one could have been nicer. I learned how they lived, hunted, and traveled—something that every arctic archaeologist should know. There was still a lot to be discovered in the North, and the by-products were sometimes as important as the archaeology. We completed the map of Baffin Island, rediscovered a route across it, and added two islands totaling about 1,000 square miles to Canada. I should add that, though they were new to us, the Inuit had known about them for generations. There were other by-products. For example, the Inuit brought me some archaeological specimens wrapped up in the skin of a bird they had shot. They told me it was not a common bird, and they were right. When I took it to the National Museum, it was identified as a fieldfare, a bird that was then unknown in North America.

When the war was over, I returned to Canada. I would have liked to return to arctic archaeology, but by then I had a wife and family to support and not enough money to do both. Diamond Jenness tried very hard to get a grant for me so I could write up in more detail what I had done, but without success. The Canadian government and scientific foundations had little interest in arctic archaeology in the years immediately following the war. As a result, my career became concerned with living Inuit rather than dead Eskimos. Since then, my major contribution to arctic archaeology has been to father an Eastern Arctic archaeologist, whose paper appears below. I now go back to Igloodik every summer to excavate, but I have to do what she tells me. I have to use a small brush where I would have used a snow-knife and a shovel. She is always highly critical of the way I excavate, but in those days we

were trying to paint a bigger picture, to which we are now adding ever finer detail.

As a footnote, I would like to include a short letter I found only while preparing my paper for the Elders Conference. Between the pages of a reprint of Diamond Jenness's article in the *Geographical Review*, "A New Eskimo Culture in Hudson Bay," that I had been given in 1935 was a handwritten letter from Jenness to A. C. Haddon, Reader in Anthropology at Cambridge University. It may make some of the younger archaeologists envious of their elders.

Department of Mines,
Victoria Memorial Museum,
Ottawa,
Feb. 17, 1926

Dear Dr. Haddon:

I wonder if this little article will interest you? Eskimo history grows more and more complex the more we learn, and every new theory seems to go a year later into the wastebasket. Just at present I have this feeling; that 1000–2000 years ago there flourished two Eskimo civilizations. One (Thule culture) extended from N. Alaska to Hudson Bay and N.W. Greenland; the other (C. Dorset) centred around Hudson Strait, reached to the north of Baffin Island and even to Ellesmere Land, and probably extended throughout the Labrador Peninsula. While peculiar in many respects, this C. Dorset culture shows strong Indian (Algonkin, etc.) affiliations, and probably represents the legendary "Tunnit" of modern Eskimo traditions. I have other wild notions and theories floating in my head that I dare not put to paper. But this summer, if all goes well, I hope to visit Bering Strait and do a little digging. Who knows what luck I shall have?

With very kindest regards.
Yours sincerely, D. Jenness

What he found, of course, was the Old Bering Sea Culture to add to his discovery of the Dorset.

From Alarnerk to Nunguvik

GUY MARY-ROUSSELIÈRE

When I was invited to attend the Elders Conference and give a paper, my first intention was to talk of Nunguvik. But I was told that, since this was a historical meeting, perhaps I should rather tell you how I became an archaeologist. Even though I was not completely convinced of the interest of the subject, I decided to comply. By the way, since archaeology is not my main profession, I still consider myself an amateur archaeologist.

I must say that during my classical studies I was always interested in archaeology and prehistory. Before I joined the Oblates, I studied at the Saint Sulpice seminary in Paris where these interests were a tradition. It was there that Abbé Breuil had studied, as well as two other priests, Fathers Bouyssonie and Bardon, who had discovered the first Neanderthal grave at La Chapelle-aux-Saints.

When I left France for Canada in 1938, however, I had no idea that I would one day be digging in the permafrost to find traces of prehistoric man. While living with the Dene in Northern Manitoba, I had found some stone points while preparing the ground to build my house at Little Dutch Lake. Later, Indians brought me different samples of prehistoric stone industry and even a very old pistol that might have been lost by Samuel Hearne on his way to the Coppermine River.

But it was only when I was sent to Pond Inlet in 1944 that I had the opportunity to see, for the first time, specimens of the Thule culture, which were brought to the mission by local Inuit: this was not just stone material but also carved bone and ivory pieces.



When I went to Igloolik in 1946, I spent the first part of the summer at Alarnerk, on the mainland, just south of Igloolik Island, where an important camp had been established in order to hunt walruses. Most of the people in the camp were members of the family of Ittuksarjuaq, the man whom white people used to call the “king.” He had died two years before, but his widow, Monica Ataguttaaluk, the “queen,” was still very much alive. She was a great lady and she had decided to take charge of my education.

At Alarnerk, along the low limestone coast, one could see everywhere the rectangular imprints left by the summer tents. While walking behind the camp, I could see on the raised beaches the traces of more tent rings of different shapes. But when I got to seven or eight meters above sea level and higher, the shapes became rectangular like the tents of the present-day Inuit. Monica Ataguttaaluk told me, “No, these people were not our ancestors: they were the Tunit, the people who occupied the land before them.” A few days later, I saw her digging with a few girls in a midden. In the evening, she brought me a collection of her finds. At

first sight, I could see that these pieces were very different from the Thule specimens I had seen in Pond Inlet.

It was only the following year, when Father Bazin returned to Igloolik, that I heard for the first time of the Dorset culture. In the meantime, I had learned many stories about the Tunit and became interested in the prehistory of the Arctic. I also began to think I would like to know more about these Dorset people.

A few years later, in 1949, I returned to France and met André Leroi-Gourhan at the Musée de l' Homme in Paris. (Much later, in 1968, I was to spend a day excavating with him at Pincevent and the only thing I found in that caribou hunting camp in the middle of France was rather unexpected: it was a shark tooth. Leroi-Gourhan encouraged me to find out more about the Dorset culture.

When I returned to Canada, I was sent to Baker Lake and later to Repulse Bay, where I visited Naujan and Aivilik. I could not do any digging without a permit, but I knew that some of the other sites I found in the region must be Dorset sites, and I began to map them.

In 1953, I was sent to Churchill as editor of *Eskimo* magazine. I was still there the following year when I had a visit from Jørgen Meldgaard, who had just arrived with Dick Emerick on his way to Igloolik. Of course I told him what I knew about the Dorset site, and when he invited me to join them for the summer, I accepted.

One of the first sites that we visited was, of course, Alarnerk and that is where I got my training. I would like to mention that, besides Jørgen and myself, there was also one other person, whom I would certainly call a junior member of the expedition. He is the son of my late friend Pacome Qulaut, who was our guide, and he arrived a few weeks after us; more precisely, he was born there. During that interesting summer, I found more important sites across Foxe Basin.

In the following years, I excavated at Baker Lake and mostly at Pelly Bay, before going to the Université de Montreal in 1962 to study anthropology. When in

Pelly Bay, I had made a collection of string figures, perhaps the most comprehensive for a single Inuit settlement. In doing so, I was not leaving archaeology. As a matter of fact, I consider string figures as living archaeological specimens. Indeed, a string figure is not only a material figure but it comes with a name and sometimes a story. And, more than once, I found in Alaska the meaning of a name that had been forgotten on the way to Pelly Bay.

When I was sent to Pond Inlet in 1958 to take care of the mission, I had only a small congregation and did not lack time for archaeological investigation. I began to dig at the well-known site of Button Point and was able to send the National Museum an interesting collection of wooden carvings. Unfortunately, the stratigraphy was much disturbed by solifluction. It is there, when I was sure of having at last found a pure Dorset midden, that I discovered a pipe stem with the words "McLean, Dundee"!

At Mittimatalik, I also excavated the grave of Mitima, the man who gave his name to the village, and found that he was not alone. Lying parallel to his body but in the opposite direction were the remains of a younger woman. A few years later, just a few meters from the Thule house excavated by Matthiassen, I found the oldest Pre-Dorset harpoon heads in North Baffin.

I explored many other sites of the region—one of them had the very enticing name of Tunit and I went there dreaming of finding the grave of the last Tuniq, but I did not even find a single microblade. Later that summer, I came to the most interesting of all: Nunguvik. There, I found a place that was completely different. At Alarnerk, one could just go from one raised beach to the next and move back one century or so at the same time. At Nunguvik, on the contrary, the altitude did not mean much, because the sea level had not changed during the last 2,000 years. It meant that, in a place like House 73, which I prefer to call a complex, the archaeological layer reached up to 80 centimeters, and, below 25 centimeters, much of the bone and wooden material was well-preserved. I found

there not only stone tools similar to the ones we had found at Alarnerk but also their wooden handles. Of course, it implied that this particular place had been inhabited for several hundred years and that some of the specimens could not be dated with as much precision as those at Alarnerk.

There was also another difference. Alarnerk had been a settlement of walrus and seal hunters, while at Nunguvik caribou dominated the culture and caribou bone was the source of most of the local industry. It meant that, with the partial exception of the harpoon heads, the tool kit of the Nunguvik people was very different from that of the Alarnerk people and of most other well-known Dorset sites. The result of having excavated more than 130 square meters is that N73 has produced one of the most complete inventories of the Middle Dorset period in one region, including wooden ski miniatures and parts of kayaks.

Another interesting point was that, while the caribou bone tools were mostly made in Nunguvik, as shown by the great number of reject pieces, they were mostly used at Saatut, a fishing and sealing camp situated twenty-five miles to the south in Eclipse Sound.

Most radiocarbon dates coming from Nunguvik are generally later than those coming from other sites with similar harpoon heads. Whether that is the sign of a time lag or not is unclear, but one should remember that the dates for T-1, Tyara, and early Dorset sites of the Lake Harbour region came originally from marine animal samples. At least, Nunguvik has given two dates for the same kind of sample. The inhabitants of the last Dorset house had already learned from the Thule people how to make fire by rotating a piece of wood.

There are still some problems to solve in the region. For instance, the presence in the oldest Dorset house of Nunguvik (N46), dated to 350 B.C., and the nearby site of Arnakadlak (1500 B.C.) of what I call, along with Meldgaard, "mini-burins" and what Henry Collins at T-1 called "micro-burins."

Among the many puzzling pieces found at Nunguvik, I also have to mention the presence, at the bottom of a crack in House 73, of a piece of wood with iron nail marks, dated to 1280 A.D. I am quite sure that the people who will be excavating at Nunguvik after me will find more surprising things.



Gathered on the deck of the Smithsonian's research vessel Tunuyak in 1980 at Nain, Labrador are (front, L to R): Douglas Sutton, Bryan Hood, Susan Kaplan; and (rear L to R): William Fitzhugh, William Ritchie, Morten Meldgaard, Eric Loring, and Stephen Loring.

part 3

THE FAR NORTHEAST: ARCHAEOLOGY IN QUEBEC,
THE MARITIMES, AND LABRADOR



13.1/ Nukasusutok 2, Structure 2, view north from beach crest

Nukasusutok 2 and the Paleoeskimo Tradition in Labrador

WILLIAM W. FITZHUGH

This chapter explores the transition from Pre-Dorset to Dorset culture in what is sometimes referred to as the “Transitional Period” (ca. 3000–2200 B.P.) or the Pre-Dorset/Dorset transition of the Paleoeskimo tradition. This period has been a subject of long-standing interest in Eastern Arctic archaeology as one of the two major culture changes of the 4,000 years before European contact. This subject is explored from the vantage point of a Late Pre-Dorset site at Nukasusutok Island south of Nain in north-central Labrador. In addition to addressing chronological and technological change, Nukasusutok sheds light on social organization, settlement systems, seasonality, and regional processes of the Early Paleoeskimo period.

The existence of a transitional phase between the earliest Paleoeskimo, or Pre-Dorset, cultures and the succeeding Dorset culture has been recognized since the early 1950s when Eigil Knuth and Henry Collins, working respectively in Peary Land and Southampton Island, defined the parameters of the period. Knuth’s Independence II sites with their mid-passage dwellings and Early Dorset-like tools (3100–2400 B.P.) and Collins’s T1 Early Dorset collections from Native Point (2500–2100 B.P.) bracket a culture change that has been variously interpreted as *in situ* development from Pre-Dorset (Taylor 1968) or as having been stimulated by contacts and introductions from Western Alaska (Collins 1951b:428; Giddings 1957; Harp 1964a) or from the boreal forest zone of Eastern Canada (Meldgaard 1960b, 1962). While the forest theories



were short-lived and the Pre-Dorset to Dorset continuity hypothesis has become widely accepted, the causes, processes, and demographics of the change remain one of the most interesting problems in Eastern Arctic archaeology (Hood 1998b; Maxwell 1985:111–125; Nagy 2000b:1–19).

Defining the Transition

In early 1976, when a Smithsonian team investigated a small site at Nukasusutok (HcCh-5) near Nain (Cox 1978:104), our knowledge of Early Paleoeskimo culture was limited. Collins (1956a, 1956b) had identified a chronological sequence at the Native Point Dorset sites, and Helge Larsen’s and Jørgen Meldgaard’s (1958) work at Sermermiut, Meldgaard’s (1960b, 1962) at Igloodik, Eigil Knuth’s (1966–1967) in Peary Land, and Harp’s work in Hudson Bay (1997) had produced evidence of chronological change. William E. Taylor, Jr. (1968), however, was the first to systematically explore the relationship between Pre-Dorset and Dorset

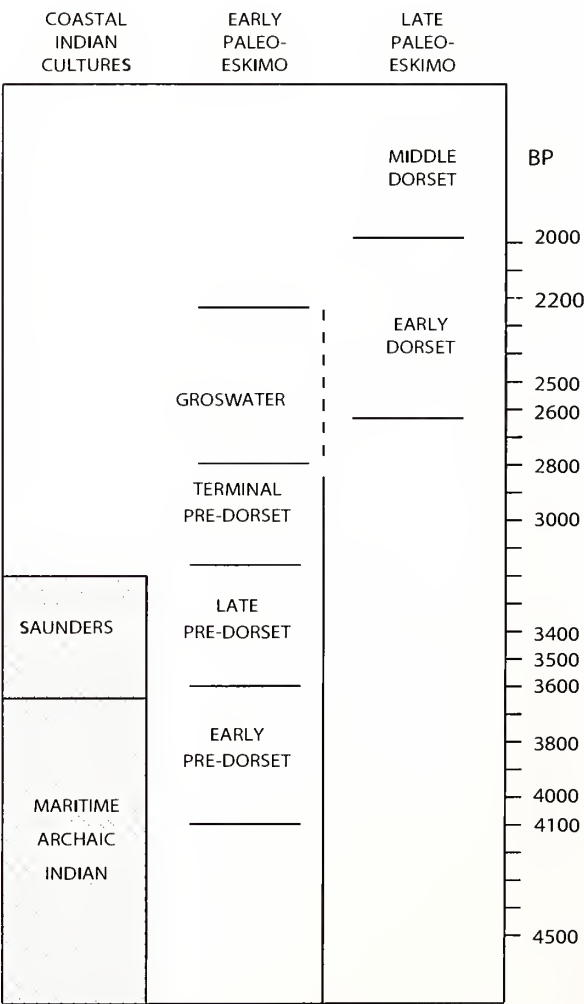
culture. Although the Eastern Arctic lacked a large database at that time, his analysis of the Late Pre-Dorset Arnapiik and Early Dorset Tyara collections from northern Ungava argued for an in situ development from Pre-Dorset to Dorset culture with little, if any, stimulus from Alaskan or southern Indian contact.

Taylor's was the most detailed study of this problem until the mid-1970s when Charles Arnold (1981) explored it and Early Dorset relationships with Alaska in his analysis of the Lagoon site from Banks Island in the Western Canadian Arctic. Later, in the early 1990s, Murielle Nagy (1994a, 2000b) conducted a study of the transition in the same area that Taylor had worked. In her opinion, the proliferation of transition-period data since Taylor's study had generated more confusion than light because of a heavy reliance on culture ecological and evolutionary models; views that were too constrained by regional and personal perspectives; inconsistent use of terminology and classification; and imprecise phase and dating assignments. Nevertheless, despite her criticism of the ad hoc nature of the scholarly process, her study concluded that the concept of a Pre-Dorset/Dorset transition was a valid chronological and cultural stage of a single broader Paleoeskimo cultural tradition (Nagy 2000b:115). She also acknowledged the need for a more comprehensive review of this transition encompassing the entire Eastern Canadian Arctic and Greenland.

Eastern Arctic archaeology has long been plagued by the lack of well-defined regional phases. It was once thought that Meldgaard's geographically central Igloodik data would serve as the standard for a sequence against which regional developments could be compared, following the implications of the core area model described below (Fitzhugh 1976b:147; Maxwell 1976b:4-5, 1985:50; McGhee 1976b). However, in the absence of the publication of Meldgaard's work and with a growing body of data from "peripheral" regions of the Eastern Arctic that suggest the existence of multiple "core areas," most researchers have questioned the validity of a unified "core area" model

of Paleoeskimo culture change. In this view interactions between multiple core areas are more important drivers of regional development than are relationships with a single nuclear area (e.g. Cox 1978:115; Helmer 1991:315-16; Odess 1998, this volume; Schledermann 1978b). A more interactive, regionally variable core-periphery model responding to climatic and environmental change and predator-prey relations now seems to fit existing data better than the original model of a single pulsating core region in northern Foxe Basin developed at the School of American Research seminar in 1973 (Maxwell 1976c).

Although peripheral to the Central Canadian Arctic, sites dating to the Pre-Dorset/Dorset transition (ca. 3000-2500 B.P.) have been found in several locations



13.2/ Framework of Labrador Culture History as discussed in this paper.

in northern Labrador. As in other regions of the Central Arctic and Greenland (Andreasen 1997), the major problem in understanding this period in Labrador has been the paucity of late Pre-Dorset components.

Paleoeskimo Systematics in Labrador

Attempting to rectify a practice of ad hoc classification in archaeological systematics, Knuth (1977-1978), Plumet (1982), McGhee (1982a, 1996), Maxwell (1985, 1997), and Helmer (1994) have presented various taxonomies. The system used by Smithsonian researchers to classify Eskimo cultures is similar to that presented by Helmer (1994:fig.1), and is seen here as Figure 13.2. The only major departure from Helmer is the placement of transitional Groswater/Independence II cultures. Most researchers working in the Central and High Canadian Arctic consider the "transitional" Independence II phase as a Dorset rather than a Pre-Dorset culture. In recent years, archaeologists working in Labrador and Newfoundland have argued that Groswater and its northern variant, Independence II, are best understood as Early Paleoeskimo cultures whose technology and tool styles demonstrate a transition from Pre-Dorset toward Dorset but whose settlement systems and economies are still Pre-Dorset (Early Paleoeskimo) in nature (Cox 1978:104; Fitzhugh 1980b; Loring and Cox 1986:78; Tuck and Fitzhugh 1986:164).

The Labrador Paleoeskimo tradition spans the period ca. 4100-500 B.P., from the earliest Pre-Dorset to the latest Late Dorset sites. As an entity, the Paleoeskimo tradition in the Eastern Arctic has developed as an autochthonous tradition, despite its coexistence with Indian cultures along its southern forest frontier. Early theories of "forest" (i.e., Indian) influence (Collins 1962; Meldgaard 1962) on Paleoeskimo technology, settlement forms, and adaptations have been rejected. It appears that social boundaries between Paleoeskimo and Indian peoples to the south were actively, perhaps even aggressively, maintained and defended (e.g., Fitzhugh 1972b:180-197, 1987:149). On the other hand, at certain times contacts between Eastern and

Western Arctic Paleoeskimo groups may have occurred. Similarities and dating correspondences between Early Dorset and Late Choris and Early Norton cultures in Alaska, including such features as soapstone vessels, side-blades, ground burin-like tools, and the use of ground slate, nephrite, jade, and semisubterranean houses (Arnold 1981:159; Giddings 1957, 1960:172, 1964; Harp 1964a:157-163) may indicate some type of Alaska involvement in Dorset origins.

The Paleoeskimo tradition is usually divided into two major segments, Early Paleoeskimo (EPE) and Late Paleoeskimo (LPE), separated by a Transitional Horizon that includes elements of both (Maxwell 1997). The EPE tradition in Labrador has three culturally and chronologically distinct units or phases: Early Pre-Dorset (EPD), dating ca. 4100-3500 B.P.; Late Pre-Dorset (LPD), dating ca. 3500-3200 B.P.; and Terminal Pre-Dorset (TPD), dating ca. 3200-2900 B.P. (Table 13.1). EPD sites are numerous on the coast north of Voisey's Bay and have consistent technological and raw material usage patterns (Cox 1978). Late Pre-Dorset develops from Early Pre-Dorset, but its sites are quite rare. Some of the diagnostic features of Late Pre-Dorset technology include the presence of relatively small burins that have ground faces, angled shanks, and hafting notches; the introduction of large triangular endblades, eared scrapers, notched bifaces; and an increase in microblade production. The largest body of LPD data currently available is from the Okak region (Cox 1977, 1987, 1988). Late Pre-Dorset sites are rare in northern Labrador, perhaps due to their loss from submergence and erosion in areas north of Saglek (Clark and Fitzhugh 1991; Fitzhugh 1980b). Their rare occurrence south of Okak, where uplift still exceeds the sea-level rise, is more likely a result of a social boundary with Saunders phase (Intermediate) Indian groups, whose sites have been found as far north as Nain and Okak.

The third chronological phase of the EPE tradition, Terminal Pre-Dorset, dating ca. 3200-2900 B.P., is even less well-known in Labrador than the preceding Late Pre-Dorset phase, and it is not certain that TPD

Table 13.1/ Labrador Paleoeskimo Classification

Tradition	Sub-Tradition and acronym	Phase/Culture and acronym
Paleoeskimo [PE] (4100–500 BP)	Early Paleoeskimo [EPE] PRE-DORSET CULTURE (4100–2900 BP)	Early Pre-Dorset [EPD] (4100–3500 BP) ¹
		Late Pre-Dorset [LPD] (3500–3200 BP) ²
		Terminal Pre-Dorset [TPD] (3200–2900 BP) ³
	Transitional Paleoeskimo [TPE] (2900–2200 BP)	Groswater (2900–2200 BP) ⁴
	Late Paleoeskimo [LPE] DORSET CULTURE (2500–500 BP)	Early Dorset [ED] (2500–2000 BP) ⁵ Middle Dorset [MD] (2000–1500 BP) ⁶ Late Dorset [LD] (1000–500 BP) ⁷

1. Selected sites: Tinutjarvik 1, Brownell Point 1 (Fitzhugh 1980b); Rose Island Q Band 4, Upernavik Site K (Tuck 1975b, 1976b); Nulliak Cove 1, S25 (Fitzhugh 1984b); St. John's Harbor 3, 4 (Thomson 1986); Okak 6 (Cox 1977, 1978); Thalia Point 2, Area 19 (Fitzhugh 1976b); Dog Bight L5 (Fitzhugh 1976b, 1976c; Cox 1978).

2. Okak 5 (Cox 1977, 1978).

3. Nukasusutok 2 (Fitzhugh 1976b; Cox 1978); Shoal Cove 4, Nuasornak (Cox 1988).

4. Big Falls (Tuck 1975b); Ticoralak 2-5, East Pompey Island (Fitzhugh 1972b); Buxhall, Thalia Point 2 A19, Forteau Bay 5 (Fitzhugh 1976b); Postville Pentacostal (Loring and Cox 1986); Phillips Garden East (Renouf 1994); Blanc Sablon (Pintal 1994); Cape Ray Light (Devereux 1966).

5. Komaktorvik 1 (Nagle 1986); Rose Island 2 (B2), Upernavik J (Tuck 1975b); Illuvektalik 1 (Cox 1977, 1978); Dog Bight L3 (Fitzhugh 1976b; Cox 1978); Dog Bight L3 (Fitzhugh 1976b; Cox 1978); Nukasusutok 12 (Hood 1986).

6. Koliktalik (Fitzhugh 1976b, 1976c); Avayalik 1 (Jordan 1980).

7. Okak 3 (Cox 1978); Dog Bight L1 (Fitzhugh 1976b; Cox 1978); Avayalik 1 (Jordan 1980).

originated from a local predecessor. Sites assigned to TPD in Labrador include Nukasusutok-2 (Nuk-2) from Nain, Shoal Cove-4 in Seven Islands Bay, and components of the Nuasornak site on Okak Bay excavated by Steven Cox (1987, 1988).

Following the TPD period and beginning ca. 2800 B.P., the Groswater phase becomes a strong presence in Labrador and adjacent regions of Newfoundland and the northeastern Gulf (Fitzhugh 1976b, 1980b; Loring and Cox 1986; Pintal 1994; Renouf 1994; Tuck and Fitzhugh 1986). While its origin seems coincident in all of these locations, its disappearance displays a time lag to the south, ending first in northern Labrador ca. 2400 B.P., on the central coast ca. 2200 B.P., and in Newfoundland and the northeastern Gulf of

St. Lawrence ca. 1900 B.P. Its distribution includes most of the Eastern Arctic and Subarctic from the Lower North Shore and Newfoundland, west to central Hudson Bay and Coronation Gulf, and north into North and East Greenland. Labrador Groswater peoples continue to follow EPE settlement patterns and adaptation systems, using surface axial structure dwellings. They appear to have used a generalized economy featuring winter caribou hunting and fishing on the near interior, rather than the intensive winter coastal settlement and hunting practiced by Late Paleoeskimo peoples. On the other hand, the Groswater technology of side-notching, ground burins, plano-convex end-blades, extensive microblade use, and soapstone lamps includes precedents that appear as characteristic features

of the subsequent early phase of the LPE or Dorset tradition. For this reason southern Groswater and northern Independence II are usually considered to be regional variants of a truly transitional culture in the Pre-Dorset–Dorset sequence.

Nevertheless, Groswater does not appear to be directly ancestral to later Dorset culture in Labrador. The LPE tradition here begins ca. 2500 B.P. when T1-like Early Dorset culture appears in northern Labrador. In addition to the new technological forms noted above, Early Dorset brings major changes in settlement, including the appearance of sod houses and middens, new lithic material use patterns, and an intensified year-round maritime adaptation. Because a distinct Groswater culture continued to persist in central and

southern Labrador, the Strait of Belle Isle, and Newfoundland for several centuries after the arrival of Early Dorset in northern Labrador, we interpret the appearance of Early Dorset as the arrival of new traditions and new peoples. The earliest Labrador Early Dorset dates (2500 B.P.) are coeval with those at the Early Dorset T1 site in Southampton Island. After several hundred years during which Early Dorset expanded south into Newfoundland and the Gulf, replacing Groswater (and perhaps mixing with it to some degree in Newfoundland), a gradual transformation from Early Dorset to Middle Dorset occurred with minor style shifts and technological innovation. This period is marked by a growing economic and settlement orientation to maritime resources, the development of semisubterranean winter houses, the accumulation of deep and (in northern Labrador) frozen middens, and an expansion of trade networks between Newfoundland, Ungava, and the Central Arctic.

After 1500 B.P., Middle Dorset culture disappeared from the central Labrador coast at the same time that the Daniel's Rattle Indian phase (Loring 1985, 1988a, 1992) expands north into these territories. This may account for the absence in Labrador of the late Middle Dorset longhouse complex, which is found in sites of this period in most other areas of the Canadian Arctic. Dorset reappears in Labrador ca. 1000 B.P. in a Late Dorset form similar to that known elsewhere in the Central Arctic; it flourishes in northern Labrador as far south as Nain, and continues to occupy this region for the next 350 years, until it is replaced by southward-advancing Neoeskimo Thule groups (Fitzhugh 1994b; Kaplan 1980, 1983).

As mentioned above, the weakest link in understanding the Labrador Paleoeskimo sequence is the Transition Period (ca. 3200–2200 B.P.) when EPE Late Pre-Dorset culture was developing into Groswater and LPE Early Dorset culture. A better definition of TPD between 3200 and 2800 B.P. is needed to resolve these problems. Sometime toward the end of this period, a new, highly focused transitional cultural complex took

shape and spread widely throughout the Eastern Arctic in the form of Independence II, the Igloodik 22 to 23 meter terrace sites, Groswater, and other regional cultures. Shortly thereafter, ca. 2500 B.P., a new set of forces or impulses crystallized into the LPE Dorset tradition. Major features of the Transitional Period include: dates and appearances of transitional cultures in Newfoundland and Labrador that are nearly identical to those from the Central Arctic; the rapid spread of a horizon-style group of cultures, including Groswater and Independence II; a time-phased Early Dorset intrusion into Groswater territory in Labrador by a group that may have been ethnically different; and a strong possibility that Indian cultures significantly influenced LPD and MD/LD cultures and population movements. While acknowledging the validity of the concept of a "tightly-constrained" Paleoeskimo tradition when viewed in terms of technology and tool styles (Nash 1976), Transitional Period cultures exhibit dynamic demographic and economic responses to social and environmental change. These responses are especially evident as changes in culture area and territory. The possibility of such vitality and response to external social and environmental forces has been ignored in most reconstructions of early Eastern Arctic prehistory.

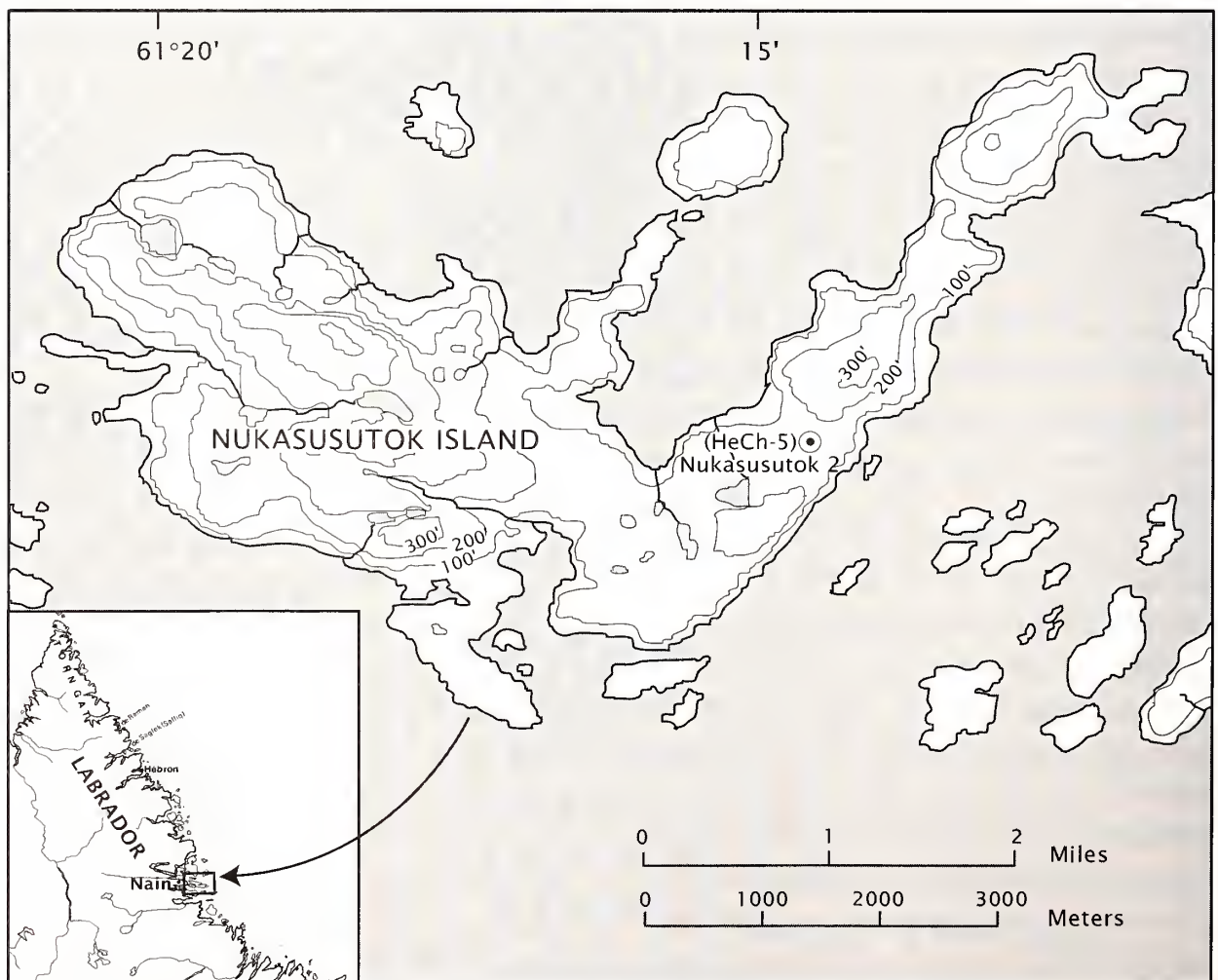
Contradictions: Continuity and Change

Until recently, continuity and change have been the dominant issues in Paleoeskimo studies. Studies of radiocarbon-dated lithic tool assemblages have been used to construct cultural sequences that revealed long-term continuity within the Paleoeskimo tradition. However, discontinuities have also been noted that cannot be easily reconciled with the "steady-state" or "gradualist" paradigm that has dominated most approaches to Paleoeskimo prehistory (Maxwell 1985:244; Nash 1976).

The view of Eastern Arctic Paleoeskimo continuity has been heavily influenced by perceptions of environmental conditions that most archaeologists see as being relatively stable and biologically unproductive

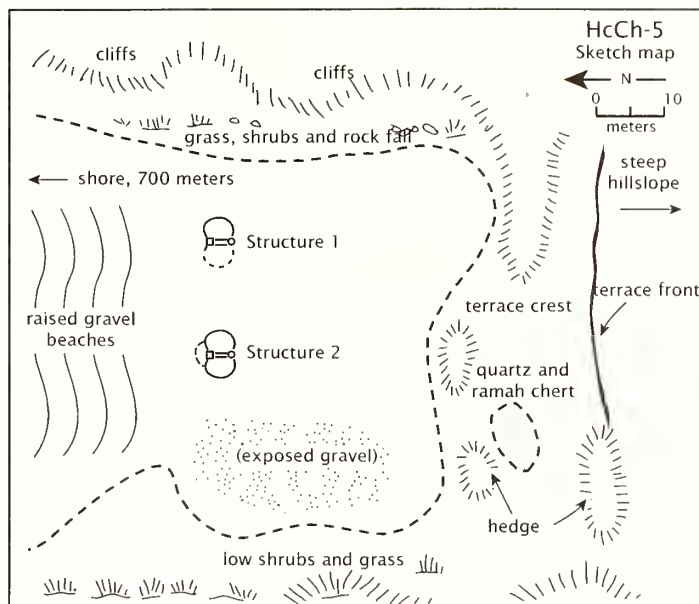
compared to the more dynamic, diverse, and biologically productive view of the Western Arctic environment. As a result, gradualism has become almost a dogma in Eastern Arctic Paleoeskimo studies. In northwestern Alaska, several distinct cultures—Denbigh, Old Whaling, Choris, Norton, Okvik, Old Bering Sea, Ipiutak, and others—are recognized during the period ca. 4500–1000 B.P.; by comparison, during the same period of time, the Eastern Arctic Paleoeskimo tradition includes only two cultures, Pre-Dorset and Dorset, and possibly a third if one accepts the Transitional Period group as a distinct culture. While our ability to detect diversity in Alaska is influenced by the presence of pottery, distinct art styles, and greater settlement diversity, the less highly styled cultural profiles of the Eastern Arctic have reinforced the view

of Eastern Paleoeskimo as a single, slowly developing tradition with relatively little internal diversity and few external stimuli from the Western Arctic, the boreal forest, or the northwestern Atlantic coastal zone. Evidence supporting the gradualist view is found in Paleoeskimo adherence to a single homogeneous technological tradition, tool styles, settlement types, and subsistence adaptations that cut sharply across the region's physical and biological diversity, creating the impression of a single culture evolving slowly over a huge geographical region. Exceptions and gradations exist, primarily at the peripheries, as in Newfoundland and Greenland; but the course of Paleoeskimo prehistory overall has been seen as a single, slowly emerging pan-regional tradition (Maxwell 1985; McGhee 1996:70, 174).



13.3/ Map of Nukasusutok Island.

On the other hand, the spread of tool styles, technological developments, and the wide-ranging recovery of distinctive raw materials, including chert, musk ox hair rope, soapstone, nephrite, and many other materials far from their places of origin, suggests widespread social networks at various times during the Paleoeskimo tradition. This evidence points to a dynamic, interconnected culture that does not appear seriously constrained by environment or external factors. The conclusion of this paper will discuss how to accommodate these seemingly opposing points of view.



13.4/ Sketch map of Nukasusutok 2 site area

Transition Processes

What can be said about the process of culture change that established the Groswater phase in Labrador? Did it develop from the local Late Pre-Dorset culture that occupied northern Labrador at the time of the retreat of the Maritime Archaic from its northern range, or did it originate from the Pre-Dorset cultures of the Central Arctic, East Baffin, or Greenland? The crux of this problem lies in the period between ca. 3200 and 2800 B.P. Few sites have been found dating to this period in Labrador, and those that are known have small artifact samples and are difficult to link directly to other Central Arctic Late Pre-Dorset or Greenland Sarqaq complexes (Grønnow 1996; Larsen and Meldgaard 1958).

The first site dating to this period in Labrador was found in 1975 on Nukasusutok Island on the outer coast 20 km southeast of Nain. Consisting of a pair of well-preserved structures and a small lithic tool sample situated on a high, wind-swept terrace, Nuk-2 provides a rare glimpse into the past; its remains are so clearly observed that one can imagine being part of a small band of pioneers camped on a seaward island at the dangerous southern edge of the “Eskimo” world some 3,000 years ago. In addition to its chronology and tool remains, when it was found in 1975, Nuk-2 was one of the first sites in Canada to contain

Independence II “mid-passage” structures of the type identified in Peary Land, North Greenland, by Eigil Knuth. Nuk-2 offered an opportunity to explore the Pre-Dorset-Dorset transition from the perspective of migration or local cultural development.

The Nukasusutok 2 Site

During the summer of 1975, Stephen Loutrel, a wilderness yachting enthusiast, organized a sailing cruise to Cape Chidley, northern Labrador, in his sloop, *Lacerta* (Loutrel 1975). Loutrel offered a berth to Warren Hofstra, who had taken part in Smithsonian field projects in the early 1970s. During a visit to Nukasusutok Island, meaning “the place where the brothers quarreled” in Inuktitut (Wheeler 1953:62–63), Hofstra reported two unusual tent rings on a high beach on the island’s northeastern arm (fig. 13.3).

Nukasusutok 2 (HcCh-5) is located 500 m from the northern shore, 21 m above sea level near the crest of a 30 m high beach pass. The north side of this beach slopes gradually to a protected bay while the south side descends steeply in crags and ledges to the sea (figs. 13.1, 4). The setting is unusual in that almost all coastal archaeological sites in Labrador, except hunting blinds, fox traps, and a few other specialized sites,



13.5/ Bipod photograph of Structure 1 before excavation

are found within a few meters of their contemporary active shoreline, and those found on high raised beaches—such as early Maritime Archaic sites—are extremely old (Clark and Fitzhugh 1991; Fitzhugh 1972b:24-34). Nuk-2 was also unlike other coastal sites in Labrador in that it was situated in a cleft between two hills that gave it a poor view of the surrounding waters and made it essentially invisible to travelers by boat or over the ice. As revealed in Hofstra's sketches, the site's architecture—two bilobed tent rings with axial passages and stone boxes made of vertically set slabs—was also unique for Labrador and resembled Independence I and II houses described by Eigil Knuth (1954, 1966-1967, 1967) from Peary Land. In 1975, almost nothing was known about Early Paleoeskimo house types in Canada, although Harp had recently found some mid-passage houses in Richmond Gulf and

on the Belcher Islands (Harp 1975, 1976b). The presence of a Peary Land house type in Labrador would be important because previous research at Thalia Point (Fitzhugh 1976b) and Saglek (Tuck 1975b) suggested lithic tool similarities with Independence I. Even closer ties were evident between Labrador Groswater and Greenland Independence II tool assemblages. Given the limited knowledge of the day, these similarities seemed to link Labrador's Early Paleoeskimo cultures more closely to North Greenland than to the Central Arctic (Fitzhugh 1976b).

Dwelling Structures

The two dwellings excavated in 1976 were built on a coarse gravel beach 25 m north of, and 9 m lower than, the beach crest. The general configuration of the houses is of oval, bilobed dwellings whose presumed skin tent walls were held down with large

rounded rocks that had been carried from the active beach far below the site. The interior rocks were angular slabs taken from nearby outcrops. The Structure 1 and Structure 2 axial passages were parallel to each other, 14 m apart, and perpendicular to the gently sloping beach. Both houses were similar in size, 5 m wide and 4 m from front to back. The axial hearth feature of the northernmost dwelling, S1, was at first partly obscured by rocks that had been taken from the perimeter wall and piled in the center of the dwelling, leaving only a small part of the eastern ring intact (fig. 13.5). This rock pile may have been intended as a cache, although no evidence of cached materials was found; perhaps it was only meant to secure the tent cover after the house had been abandoned. Beneath the rocks we found the remains of a rectangular construction edged with vertically set slabs (fig. 13.13).

In contrast to S1, the S2 floor was completely intact and only a few perimeter wall rocks were missing. Large boulders located beyond the outer walls probably had functioned as guy-line anchors. Both houses had flagstone paving along their southeastern, uphill (probably rear) walls, but only in S1 was the pavement bordered with vertically set slabs. In S2, the primary entry was through an antechamber at the north end of the axial hearth to either side of the hearth box. An entry at the south end of the axial pavement may be indicated by the absence of wall rocks in this part of the ring.

Both structures exhibited axial features that contained six similarly constructed architectural subunits. Each subunit begins at the north side of the dwelling with a large threshold slab. Proceeding upslope toward the rear of the dwelling, this slab is followed by an area containing a four-sided stone box made of slabs set deeply into the beach gravel, then by a 2 m long, 60 cm wide slab-edged compartment with three internal subdivisions, and finally with a second large slab at the south (upslope) end of the axial feature. The 50 by 75 cm standing box at the south end of Structure 1 had been made of slabs 7 to 15 cm thick whose exposed portions rose 25 to 35 cm above the beach gravel. The Structure 2 box (fig. 13.6) was intact with a floor of small thin slabs. Its close-fitting slab walls were set 30 to 40 cm into the gravel. A large slab that tilted up against a wall rock 50 cm northeast of the box probably had served as a lid for the stone oven

or boiling chamber. Although damaged by the rocks that had been piled in the center, the Structure 1 box had a similar rectangular shape and its lid lay in the center of the crushed feature. Pavements of small round boiling stones, which must have been gathered from an active beach rather than from the angular gravel of the site, were found in the S1 and S2 hearth boxes.

Between the box hearth (Feature 1) and the rear threshold slab, each axial hearth feature contained a 2 m long by 60 cm wide compartment bordered by 2 to 3 cm thick upright slabs. This space, which functioned as a kitchen and work area, was further subdivided into three segments by 2 to 3 cm thick slabs set into the gravel as transverse dividers. The northernmost segment was a fireplace (Feature 2) for heating boiling stones in a 10 cm deep, conical slab-lined pit. In S1, we found several biface fragments, a microblade, and a set of small slabs set on edge in a rosette pattern. Beneath the slabs was a basal hearth slab lying on charcoal-stained sand. The central segment of both pavements contained a square, open hearth (Feature 3) of the type described by Knuth for Independence II houses. Its north and south sides were bordered by slabs inclined 30 degrees outwards from the base, and



13.6/ View East of Structure 2 axial passage showing (L-R) stone box hearth (Feature 1), boiling stone heating hearth (Feature 2), and lamp cooking hearth (Feature 3)

its lateral walls were made of 10 cm thick, vertically set rocks placed inside the thin outer border slabs to provide insulation and hold heat. These hearths were 10 cm deep and had slab bases that were encrusted with charred blubber and contained flakes of spruce bark and fire-cracked rock. A slab fragment with a notch in one side was recovered in the S2 hearth. As in other Paleoeskimo sites, these upright notched rocks were coated with charcoal and burned blubber stains that indicated their use as lamp or cooking vessel supports. The remaining meter of the hearth floor of both structures was paved with thin flagstones and contained no hearth deposits. Part of the S1 border edging was missing, and a pavement extended out to meet the rear wing pavement. In S2, the rear part of the hearth floor contained a bed of small cobbles. In both structures, wing pavements of thin slabs extended eastward from the hearth floor along the uphill (southeast) wall of the dwelling. A large flat rock lay at the south end of the S2 axial feature, perhaps serving as a threshold. The south end of the S1 pavement had a large boulder resting on the floor pavement between two vertical border slabs.

House Type Comparisons

While the specific features of the Nuk-2 houses are unique for Labrador, axial structures have been found at other Paleoeskimo sites in northern and central Labrador (Table 13.2). Among those dating to the Pre-Dorset period are Dog Bight L5 on Dog Island near Nain, whose axial hearth features and quadrilateral cobble hearths (Cox 1978:fig. 3a) are similar to Independence I and Sarqaq types; Karl Oom, also near Nain,

which had two isolated stone box hearths but lacked other features or artifacts; Nulliak Cove 1 S-25, which consisted of a boulder tent ring similar to the Dog Bight L1 structures with a boulder-bordered axial structure and a central hearth made of four inclined thick slabs; and Brownell Point in Seven Islands Bay, which featured a tent ring with a well-defined axial structure. To this list should be added several sites at Nuasornak Island in Okak excavated by Steven Cox (1988). From the Groswater period, axial feature structures are known from the Postville Pentacostal site excavated by Stephen Loring and Brenda Clark (Loring and Cox 1986); from Napatalik North, a tent ring complex with axial structures and artifacts north of Hopedale; and from St. John’s Island 1 in the Nain archipelago. These Groswater dwellings have central slab pavements, but they are amorphous in shape. There is no attempt to define their space with inset slabs and formal arrangements of hearths and work areas as in Independence II and Nuk-2. Early Dorset axial pavement structures have been found at Wyatt Harbor on Nukasusutok (Hood 1981a, 1986) but they were absent from a shallow Early Dorset pithouse at Komaktorvik 1.

Among these occurrences of axial hearth features, which probably occur at less than 1 percent of the known inventory of Labrador Paleoeskimo sites, none has such well-defined architectural features as Nuk-2. Formal axial hearth construction with slab-edged borders is rare in the Early Dorset and is not known from Middle Dorset sites. However, the construction of a formal axial hearth and work floor set within border stones reappears in Late Dorset culture in Labrador and in the Central and High Arctic (Cox 1978; McGhee

Table 13.2/ Early Paleoeskimo axial structure sites in northern Labrador (See also Cox 1988)

Site	Area	Borden #	Culture	Date BP	Features
Napatalik N.	Hopedale	GjCc-8	CW	n.d.	axial hearth tent ring (hereafter TR)
Nukasusutok 5	Nain	HcCh-5	TPD	3315±85	2 TRs, axial pavements, box hearths, slab insets
Karl Oom 5	Nain	HdCq-41	TPD	n.d.	2 box hearths, no axial feature or TR
Dog Bight L5	Nain	HdCh-5	EPD	3810±75	axial cobble pave., central quad. hearths
Nulliak Cove	Saglek	IbCp-20	LPD	3230±120	TR with axial cobble pave., central quad. hearth
Brownell Pt.	7 Island	liCx-2	EPD	4060±250	axial hearth TR

1981:45-55). Despite such differences as larger size houses, wider axial pavements, thicker border slabs, and different hearth forms, Early and Late Dorset axial structures utilize Early Paleoeskimo axial hearth concepts that for some reason do not appear in Middle Dorset structures.

The presence of lidded slab hearth boxes combined with the rarity of soapstone lamps and a virtual absence of soapstone pots in Early Paleoeskimo sites suggests that Nuk-2 people used a different method of cooking than Groswater, Dorset, and Neoeskimo peoples, who utilized soapstone lamps and pots extensively. The presence of sturdy, rectangular slab boxes containing small rounded cobbles and bottom slabs whose surfaces are not charred or encrusted with blubber suggest that these boxes were lined with hides and functioned as boiling chambers heated with seething stones. Eigil Knuth (1966-1967:195) found boiling stones in some of his Independence I box hearths. Similar boxes have also been found in Ellesmere and West Greenland Independence I and Sarqaaq sites (Schledermann 1990:77, 1996:62). Independence II sites in Peary Land and the Canadian High Arctic contain central axial features with stone hearths made of thin, sometimes double-walled, boxes (Knuth 1967:52) that are similar to Nuk-2 box hearths, but are placed in the center rather than on the end of the hearth feature. In eastern Hudson Bay, Harp (1997) found boot sole-shaped pieces of soapstone (a mineral with high specific heat) associated with stone hearth boxes in his Independence II-related Tuurngasiti sites in the Belcher Islands, suggesting that these unusual artifacts may have been used both as boiling stones and boot warmers.

Besides the Belcher sites, the closest parallels to Nuk-2 structures are found in Independence II houses (Knuth 1966-1967:203). Although not identical, structures from Delta Terrace, Cape Holbaek, and Lolland Lake contain axial features constructed with thin slab insets and have centrally placed, inclined slab hearths, double-walled slab insulation, wing pavements, and

oval outlines that compare closely with Nuk-2. As noted above, the Independence II stone boxes take a different form and may not have been used for "seething stone" cooking. Nor are dwellings of this period in Peary Land constructed with foyers or bilobed "figure-eight" forms. Radiocarbon dates for the Peary Land Independence II sites fall between ca. 3000 and 2400 B.P.

McGhee (1981:14-20) has reported Independence II structures from Port Refuge at the Skull and Rbjr-2 sites. These sites, which were not dated or excavated, appear to conform closely to the Peary Land forms, having axial pavement features bordered with vertical slabs, compartments, and central box hearths, but they also lack the heavy stone boxes found at Nuk-2 and at Schledermann's Buchanan Bay Transitional sites. In the latter region, Late Pre-Dorset sites have cobble-bordered axial structures and only at the Transitional Skraeling Island 5 (Feature 1) is there a suggestion of Independence II type construction, but without heavy stone boxes (Schledermann 1990:156).

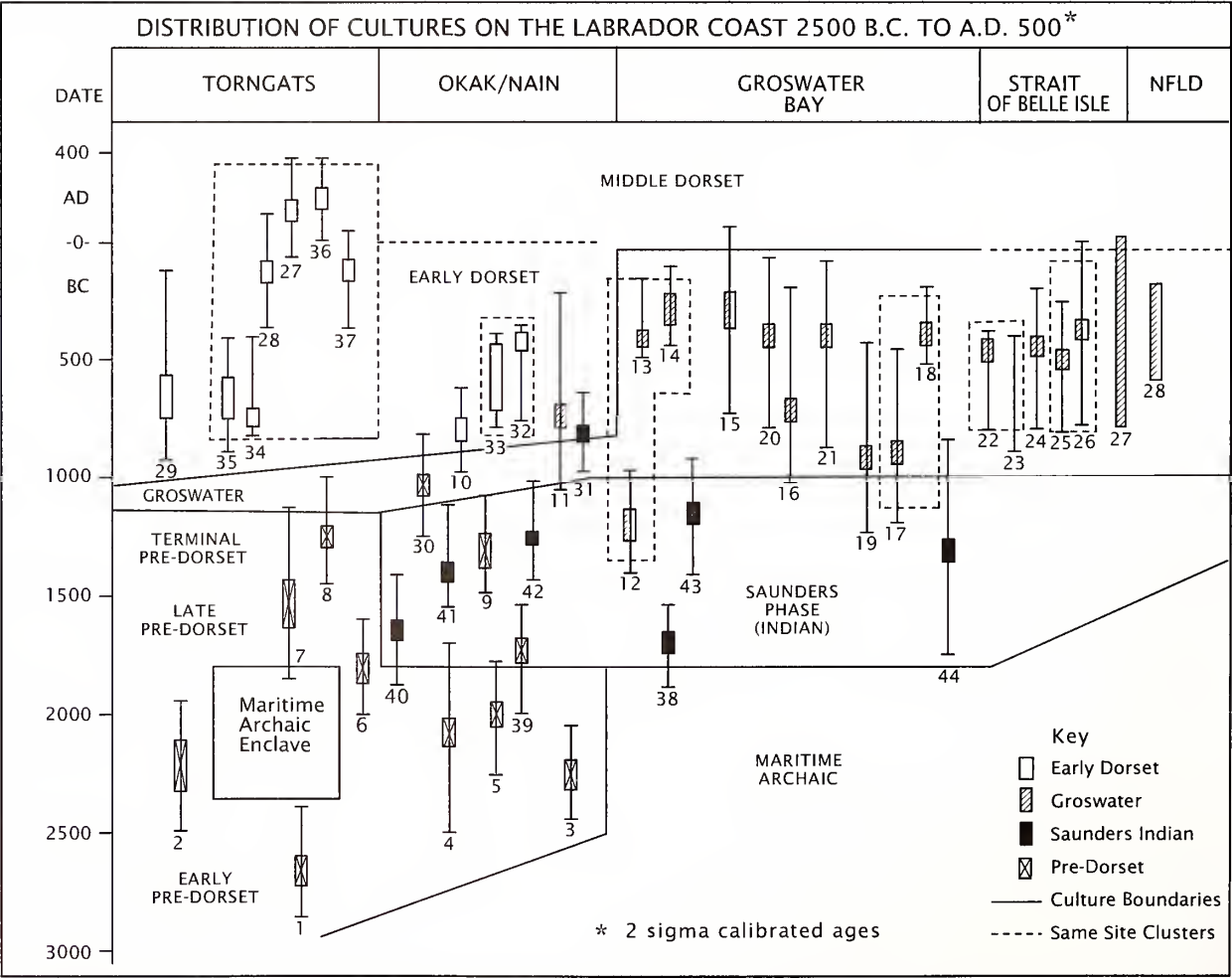
Houses similar to Nuk-2 are also known from Transitional components reported by Harp (1970, 1975, 1976b, 1997) from his Richmond Gulf and Belcher Island Tuurngasiti sites, and at Atchukaluk (HbGc-4) and Innalialuk (HaGe-3). Harp's preliminary reports indicate the presence of axial hearth constructions with central boiling boxes and inclined-slab fire hearths, Groswater-type assemblages, and 2500 B.P. radiocarbon dates. Similar finds have been made in Richmond Gulf at Atchukaluk (Gosselin et al. 1974).

From these comparisons it is clear that the interpretation of "mid-passage" houses developed by Knuth for his Peary Land sites applies with minor modifications to the Nuk-2 dwellings. Knuth (1966-1967:199) has pointed to the similarities between his Independence mid-passage dwellings and the ethnographic Sami Lappkota winter tent structures, which feature a linear hearth floor with a central fireplace separating two lateral skin-covered sleeping areas. The same pattern exists at Nuk-2. In both of the Nuk-2 dwellings, artifacts and debitage were found in or adjacent to

the hearth passage, most commonly near the southern end of the axial feature, which was paved and would not have been covered by skins or bedding. The lateral spaces in both houses had gravel floors. These areas had few slabs and contained almost no flakes, tools, charcoal, burned blubber, or fire-cracked rock, suggesting that they had been covered with hides as in the Sami case.

While they have often been called a “mid-passage hearth” or “axial pavement,” the purpose of these features in Paleoeskimo dwellings appears to have been to differentiate the “dirty” heating, lighting, and food preparation space in the center of the dwelling from the sleeping and “clean” work areas on either side of the axial hearth. Raised-slab edging helped to separate these domains. In contrast to the heavier paving

stones used as flooring at the ends of the feature, the thin 1 to 2 cm thick pavement areas within the hearth complex could not have withstood foot traffic, nor would there have been room for walking between the closely spaced clusters of hearths it contained. These areas, therefore, must have served as hearth floors and as “counters” for cutting meat, preparing foods, and making tools. Hence, at least in this Paleoeskimo context, “axial hearth” is preferable to “mid-passage” to describe this feature, and the term “axial hearth dwelling” is a more accurate term for the dwelling type than “mid-passage” house. When this form of construction reappears in Late Dorset culture, in houses that were much larger in size and with paving stones that were much thicker, the term “hearth passage” is probably an accurate description.



13.7/ Radiocarbon dates for Late Paleoeskimo and Early Dorset sites from Northern Labrador to Newfoundland (number key is found in Table 13.3)

While generally oval in shape, both of the Nuk-2 structures have a distinct bilobed form that results from the wall rocks holding down the skin coverings of the lateral sleeping “rooms” that curve in to meet the ends of the axial structure on both the north and south sides of the dwellings. Although we could not identify doorways *per se*, the presence of debitage and tool remains in two small concentrations (middens? work areas?) several meters south of S2 and the necessity for uphill entry suggests that the antechamber region facing the sea provided the primary access to the

dwelling. The bilobed shape raises questions about how the structure was constructed and covered and whether the dwelling might have had doorways at both ends of the axial hearth. The presence of a wing pavement on the east side of each house and its absence on the west side may also be significant in terms of social or work arrangements, and may also signify a rear entry capacity.

A curious feature of some Paleoeskimo dwellings is the presence of round rocks or boulders resting on the floor pavement at one or both ends of the axial

Table 13.3/ Radiocarbon dates for the Labrador Pre-Dorset–Dorset Transition (See fig. 13.7)

Key	Site Name	Culture	Laboratory Number	C14 date	Calibration
1	Little Ramah Bay	EPD	SI-4002	4055±80	BC 2885 (2586) 2406
2	Rose Island Q (Band 4)*	EPD	I-5250	3830±115	BC 2497 (2308-2295) 1944
3	Dog Bight L5	EPD	SI-2521	3810±75	BC 2470 (2289-1756) 1618
4	Thalia Point 2 A19	EPD	GSC-1264	3700±140	BC 2489 (2135-2042) 1696
5	Double Island (B Hood)	EPD	B-57125	3640±70	BC 2272 (2031-1981) 1780
6	Okak 6	EPD	SI-2507	3475±75	BC 2009 (1858-1756) 1618
7	Nulliak Cove S25	EPD	B-8701	3230±120	BC 1859 (1502) 1136
8	Shoal Cove 4	TPD	SI-3867	3005±80	BC 1450 (1293-1263) 1010
9	Nukususutok 2	TPD	SI-2988	3055±85**	BC 1500 (1378-1321) 1051
10	St. John's Island 1	GSW	SI-2990	2645±65	BC 970 (807) 604
11	Thalia Point 2 A25	GSW	GSC-1314	2540±160	BC 1068 (787-665) 215
12	Postville Pentacostal	GSW	SI-2989	2975±70	BC 1429 (1257-1135) 975
13	Postville Pentacostal	GSW	SI-3359	2275±65	BC 481 (388) 174
14	Postville Pentacostal	GSW	SI-3560	2230±65	BC 410 (359-229) 117
15	Red Rock Point	GSW	SI-875	2200±120	BC 746 (352-212) AD 51
16	East Pompey Is.	GSW	GSC-1367	2520±160	BC 1010 (767-662) 211
17	Buxhall	GSW	SI-930	2720±125	BC 1257 (893-835) 451
18	Buxhall	GSW	SI-931	2255±55	BC 410 (380) 173
19	Ticoralak 2	GSW	GSC-1179	2690±140	BC 1257 (826) 412
20	Ticoralik 3	GSW	GSC-1217	2340±140	BC 800 (400) 73
21	Ticoralak 5	GSW	GSC-1314	2400±160	BC 893 (411) 90
22	Blanc Sablon (EiBg-43a)	GSW	B-19637	2420±60	BC 790 (506-415) 390
23	Blanc Sablon (EiBg-43a)	GSW	B-40350	2570±90	BC 900 (792) 410
24	Blanc Sablon (EiBg-14)	GSW	B-19633	2400±100	BC 800 (411) 211
25	Blanc Sablon (EiBg-29a)	GSW	B-23004	2430±80	BC 800 (516-433) 380
26	Blanc Sablon (EiBg-29a)	GSW	UQ-1753	2300±150	BC 800 (393) AD 1
27	Phillips Garden East	GSW	average of 14 dates	2411	BC 759 (480-413) 401
28	Phillips Garden West	GSW	average of 7 dates	2260	BC 401 (383) 208
29	Rose Island Q, B2 (IdCv-6)	ED	I-4523	2485±185	BC 1048 (760-563) 126
30	Illuvektalik (HhCk-1)	ED	SI-2510	2845±60***	BC 1258 (1047-1011) 833
31	West Dog Is.	ED	SI-2978	2680±70	BC 1005 (823) 664
32	Dog Bight L3 (HdCh-3)	ED	SI-2153	2400±70	BC 790 (411) 380
33	Dog Bight L3 (HdCh-3)	ED	SI-2522	2455±75	BC 800 (753-448) 390
34	Komaktorvik 1 (IhCw-1)	ED	SI-3896	2515±70	BC 820 (766-662) 410
35	Komaktorvik 1 (IhCw-1)	ED	SI-3897	2495±70	BC 810 (762-595) 400
36	Komaktorvik 1 (IhCw-1)	ED	B-33048	1850±70	BC 86 (AD 132) AD 340
37	Komaktorvik 1 (IhCw-1)	ED	B-33049	2110±70	BC 380 (168-125) AD 49
38	Saunders L1	SAUN	SI-2525	3410±70	BC 1921 (1731-1695) 1530
39	Hillsbury 3	SAUN	SI-2982	3440±75	BC 2011 (1742) 1533
40	Thalia Pt. 5	SAUN	SI-2984	3320±80	BC 1873 (1679-1619) 1430
41	Thalia Pt. 5	SAUN	SI-2524	3100±75	BC 1540 (1409) 1114
42	Hillsbury 3	SAUN	SI-2981	3000±75	BC 1440 (1292-1262) 1010
43	Smooth Land Pt.	SAUN	SI-1794	2955±85	BC 1430 (1213-1116) 927
44	Red Ocher	SAUN	GSC 1280	3070±180	BC 1740 (1384-1323) 831

*All material dated is wood charcoal, except: ** blubber date corrected for c12/13, *** fat/charcoal mixture*

**Rose Island Site Q (B2) assemblage contains both Groswater and Early Dorset (incipient tip-fluting) elements and may be a mixed component.*

feature. The large rock at the south end of the hearth passage in S2 may have been used to secure a door flap, but observations at other Labrador Paleoeskimo sites suggest that rocks placed at the ends of axial features had a special, possibly ritual, function. We have found large “blocking” rocks on the thresholds of Late Dorset structures in Seven Islands Bay, and at Newell Sound in Frobisher Bay where a huge “four-person” boulder had been rolled onto the entry pavement when the house was abandoned. Knuth (1966–1967:fig. 3) also illustrates a large rock at the end of an axial feature at Lolland Lake. Keeping evil spirits from one’s house is a serious business in many arctic societies. Southwest Alaskan Yup’ik people barred evil spirits with ritual strands of grass (Reed 1982). Perhaps Paleo- eskimos used rocks to block the passage of harmful spirits into their houses when they were not in use.

Radiocarbon Samples

No charcoal was found in S1, but two samples for radiocarbon dating were recovered from the hearth areas in S2 (fig. 13.7, Table 13.3). Sample 1 consisted of burned blubber from beneath the basal hearth slab in Feature 3, the southern hearth, and sample 2 consisted of charred spruce (?) bark from below the south-east corner of the hearth slab of this same feature. Sample 1 returned an age of 3315 ± 85 B.P. (SI-2988). A C12/13 correction brings this age to 3055 ± 85 B.P. or slightly later, given its likely marine mammal source.

Table 13.4/ Artifact finds at Nukasusutok 2 (HcCh-5)*

Artifact Type	Structure 1 Number (%)	Structure 2 Number (%)	Total Number (%)
Celts		10 (.16)	10 (.11)
Burin spalls	1 (.09)	7 (.11)	8 (.11)
Burins (10 ground)	1 (.09)	10 (.16)	11 (.16)
Bifaces	4 (.36)	8 (.13)	12 (.17)
Endscraper		1 (.02)	1 (.01)
Microblades	1 (.09)	9 (.15)	10 (.14)
M-blade cores/frags	4 (.36)	16 (.26)	20 (.28)
Utilized flakes	1	36	37
Without flake tools	11 (.98)	61 (.99)	72 (.98)
With flake tools	12	97	109

* of 117 artifacts, 109 have excavation provenance.



13.8/ Tools from Structure 2 (cat. # [a] 61, [b] 68, [c] 34, [d] 37, [e] 45, [f] 70, [g] 98) and one from Structure 1 (h; cat. # 123)

Although charcoal stains were noted beneath F2 and a minute amount of burned blubber was noted in F3 of Structure 1, these samples were too small for the dating methods available in the mid-1970s and unfortunately were not collected.

Collection Description

Nuk-2 contained no bone or organic remains other than charcoal. The excavated lithic collection consists of 109 cataloged artifacts, 97 of which were found in S2 (ca. 61 diagnostic tools) and 12 (7 tools) in S1 (Table 13.4). The largest concentration of lithic tools and debitage was found outside the entrance of S2. S1 finds were restricted to the interior of the structure. In both dwellings lithics were parsimoniously used. Virtually all tools were small and heavily utilized, and debitage consisted of flakes that were 1 cm or less in size. Raw materials utilized for artifacts include grey, greenish, banded, and speckled chert from the Cape Mugford region north of Okak, quartz crystal, and metabasalt (used for celts only). Mugford chert was the predominant raw material used for biface production. Ramah chert was less common but was used for making three of the nine endblades recovered. Quartz and quartz crystal, both of which are locally available, were the predominant raw materials for chipped stone tools. Debitage collected from S2 contained 231 small

flakes of grey Mugford chert; 19 flakes and 6 chunks of quartz crystal; 12 flakes of speckled Mugford chert; 13 flakes of green Mugford chert; and 1 flake of Ramah chert. Little debitage was recovered from S1, but what was found was similar to that in S2. No soapstone or Groswater chert (of southwest Newfoundland origin) was found in either structure.

For purposes of analysis, all of the 72 diagnostic artifacts from the site are included as a single sample. Only 12 artifacts (HcCh-5:121–132) were cataloged from S1: an unground burin (figs. 13.10k, 13.12a), a grey chert burin spall, a Ramah chert biface tip (fig. 13.11g) and midsection, a chert biface tip (figs. 13.8h, 13.12q), a side-notched crystal endblade (figs. 13.11h, 13.12x), quartz microblade cores (figs. 13.11i, j), a chert microblade midsection, and a crystal utilized flake (fig. 13.11k). The entire assemblage (excluding utilized flakes) includes 10 celts and celt fragments, 10 microblades, 20 quartz crystal core fragments, some with microblade facet scars, 11 burins, 8 burin spalls, 12 bifaces, 1 endscraper, and 37 utilized flakes.

Table 13.5/ Celt Metrics from Nuk-2, Structure 2

Specimen No.	Length (cm)
2 [13.9d]	12.5
3 [13.9g]	(2.32)*
5 [13.9a]	7.9
19 [13.9e]	(3.2)
28 [13.9f]	(5.8)
29 [13.9c]	8.7
30 [n.p.]	8.7
32 [13.9b]	(4.5)

** parentheses indicate broken specimen*

Celts (n=10)

Nuk-2 celts (fig. 13.9) were made of a rough but fine-grained metabasalt rock that may have a local origin on the island. They were prepared from 9 to 15 mm thick slabs whose edges were roughly flaked or battered into shape and whose working ends were ground in two facets to fashion a rounded convex bit with a symmetrical axe-type edge (table 13.5). Lateral constriction to facilitate hafting is found only on one specimen. The Nuk-2 celts are unlike Pre-Dorset celts, which are made of *akmak* (silicified slate) flaked into a quadrilateral cross-section (Cox 1976) but are nearly identical to Groswater types (Loring and Cox 1986).



13.9/ Celts from Nuk-2 Structure 2, cat. nos. (a) 5, (b) 32, (c) 29, (d) 2, (e) 19, (f) 28, (g) 3

Burins (n=11)

The Nuk-2 burins (1 from S1, 10 from S2) are variable in form and range from specimens with fully ground distal tips and faces to those with no trace of grinding. All are right-handed and have one or more unground lateral spall scars. Specimen 75 (fig. 13.10j) is a spall from the distal end of a burin whose lateral sides are ground flat and whose tip is ground round. Specimen 93, missing its base, has ventral grinding, multiple spall removals, and a distal tip that has been spalled and lightly polished. Specimen 90 is a right-handed burin with a single spall removed and light ventral polish; its amorphous shape may result from having a rotated spall surface. Specimen 66 has ventral polish and a polished round distal end. Specimens 27, 36, and 67 (fig. 13.10e-g) are narrow-bladed burins with polished round tips. Specimen 27 has a flat base and waisted hafting constriction, while 36 has a prepared, rounded base. Specimen 76 (fig. 13.10a) is a proximal fragment of a ground burin-like tool whose hafting modification is similar to that of Groswater implement styles. Specimen 24 and 35 (fig. 13.10h, i) are small spalled burins with light polishing on their ventral surfaces. These styles are similar to burins found at the Late Pre-Dorset Shoal Cove 4 site in northern Labrador. Specimen 121 (figs. 13.10k; 13.12a) from S1 is a large unground burin with multiple spall surfaces made on a flake.

The Nuk-2 burins display considerable variation and are similar to specimens from LPD collections in Labrador, Tyara, Port Refuge, and Devon Lowlands. The single unground specimen lacks the formal preparation and trimming of EPD forms. Others (fig. 13.10e-i) have the small size, waisted bases, unground faces, and spall surfaces characteristic of LPD complexes. A few (figs. 13.10a, c; 13.12b, d, h) are larger incipient side-notched



13.10/ Burins and spalls from Nukasusutok 2, cat. nos. (a) 76, (b) 90, (c) 56, (d) 93, (e) 27, (f) 36, (g) 67, (h) 24, (i) 35, (j) 75, (k) 121, (l) 33, (m) 31, (n) 53

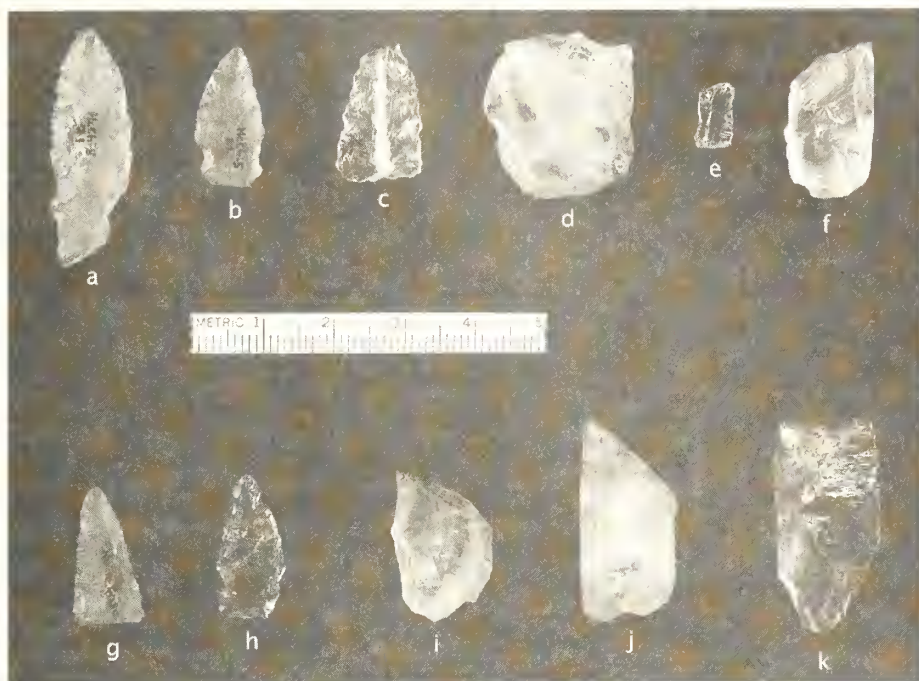
forms with ground faces, tips, and spall surfaces. One (fig. 13.10j) is a fully ground distal fragment similar to burins from Groswater components, but lacks the latter's ground spall removal surface.

Burin Spalls (n=8)

Eight burin spalls (fig. 13.10l-n) were recovered, six of which display bifacial polish and have tips ground round. One has a polished tip but no lateral grinding. Some of these spalls are larger than any of the burins recovered, suggesting that large burins had been reduced by attrition. None of the spalls have grinding on their spall scars.

Bifaces (n=12)

Six diagnostic and six undiagnostic fragments were recovered. A large biface fragment of banded grey chert (figs. 13.8a; 13.12y), a grey chert stem base (figs. 13.8d; 13.12p), an asymmetric notched Ramah chert biface (figs. 13.11a; 13.12u), and a small quartz harpoon endblade (figs. 13.11c; 13.12v) have parallels in earlier Pre-Dorset assemblages. A small notched Ramah



13.11/ Bifaces from Nukasusutok 2, cat. nos. (a) 21, (b) 22, (c) 26, (d) 62, (e) 69, (f) 57, (g) 130, (h) 122, (i) 128, (j) 125, (k) 131

chert endblade (figs. 13.11b; 13.12w) and a similar notched crystal endblade (figs. 13.11h; 13.12x) suggest Groswater forms, as does a small notched base spall (fig. 13.8e). Several undiagnostic fragments were also found. No grinding was present on any bifaces, and no plano-convex sections occurred. These bifaces resemble Pre-Dorset forms and appear transitional to Groswater types, especially the small notched knives.

Endscraper (n=1)

A single quartz crystal piece with a steeply worked distal edge (fig. 13.11 d) was found, although this may possibly be a reworked microblade core. It is probably an ad hoc type and does not conform to either Pre-Dorset or Groswater endscraper forms.

Cores (n=20)

A large number of crystal microblade cores, blanks, and core fragments were recovered. Most were small in size, ranging from 1.5 to 2.0 cm in length. Only six of these have clear blade removal scar surfaces, and many are unmodified crystals. On worked cores, an acute striking platform had been created for blade removal. No chert cores were found.

Microblades (n=10)

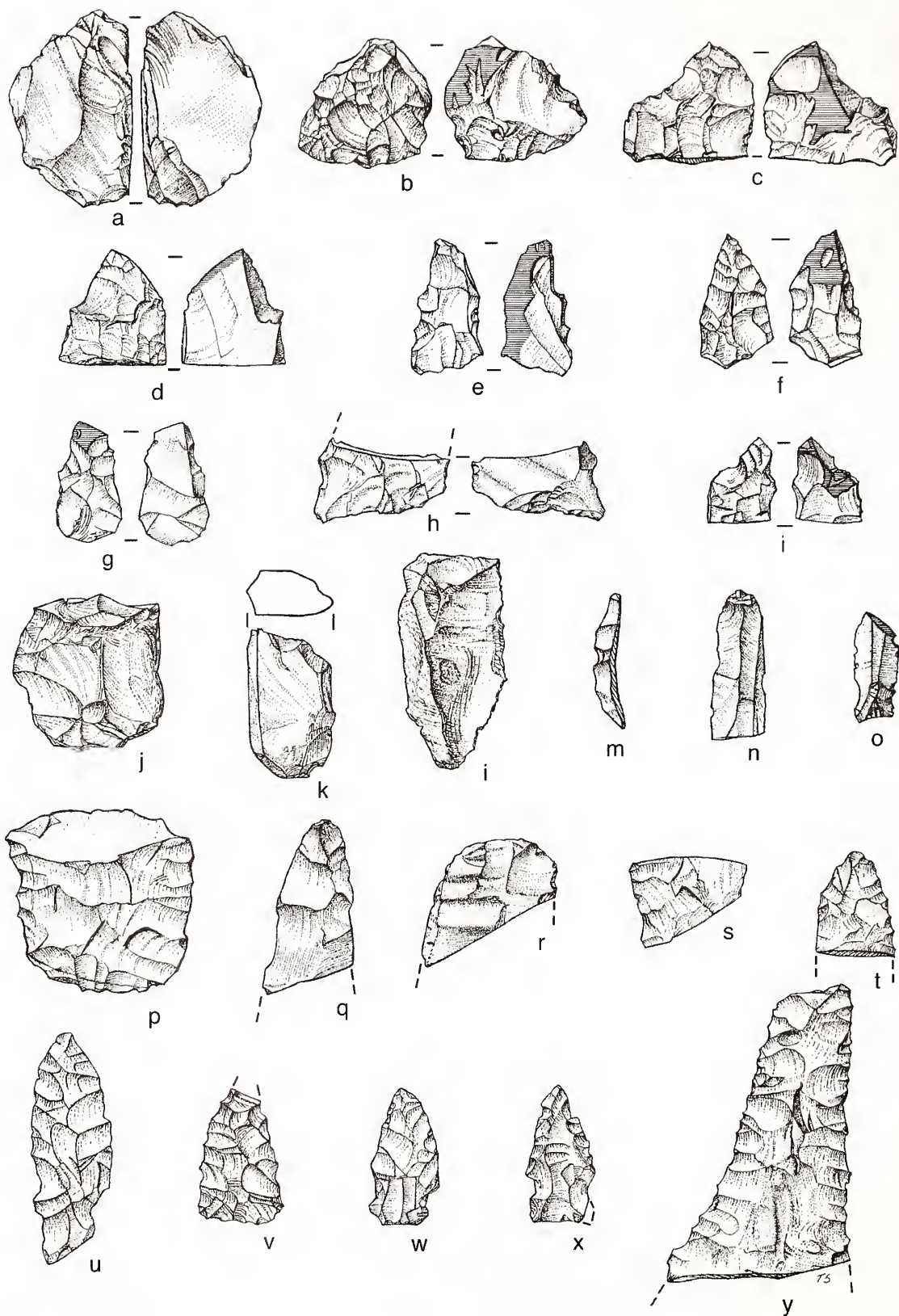
Seven crystal and three chert microblades were found. Two of the chert specimens were basally notched for hafting and one of these (fig. 13.8f) has its tip ground into a "boot creaser" tool. Most were irregular specimens with widths from 5.0 to 7.2 mm.

Flake Tools

Approximately thirty utilized flakes were recovered, but all were small, thin flakes (1.0 to 1.5 cm

wide) with limited areas of edge modification, much of which could have been caused by frost action in the coarse gravel matrix. No carefully prepared flake tools were found.

The Nuk-2 collections provide interesting comparisons with other Paleoeskimo complexes in Labrador. Technologically, this small assemblage has more Pre-Dorset than Groswater or Dorset characteristics. The scarcity of microblades, high percentages of burins and biface endblades, frugal use of chert resources, limited use of Ramah chert and quartz crystal, and dominance of Mugford cherts are all typical of Labrador Pre-Dorset technology. The presence of a single unground burin in a complex dominated by spalled and minimally ground burins is not unexpected in a late Pre-Dorset complex, although it would be in a Groswater assemblage in which spalls and burin tips and faces are always ground. The presence of asymmetric bifaces (PD), large, serrated stemmed bifaces (PD), side-notched bifaces (GW), waisted or notched small burins (LPD/GW), and broad, flat celts (GW) represent a mix of late Pre-Dorset and Groswater traits. Stylistically, Nuk-2 is still "Pre-Dorset" and lacks characteristic features of



13.12/ Drawings at 1:1 scale of selected artifacts from Nukasustok 2 showing range of tool types found at the site and the variety of manufacture techniques employed. Ground surfaces are indicated by fine hatching.

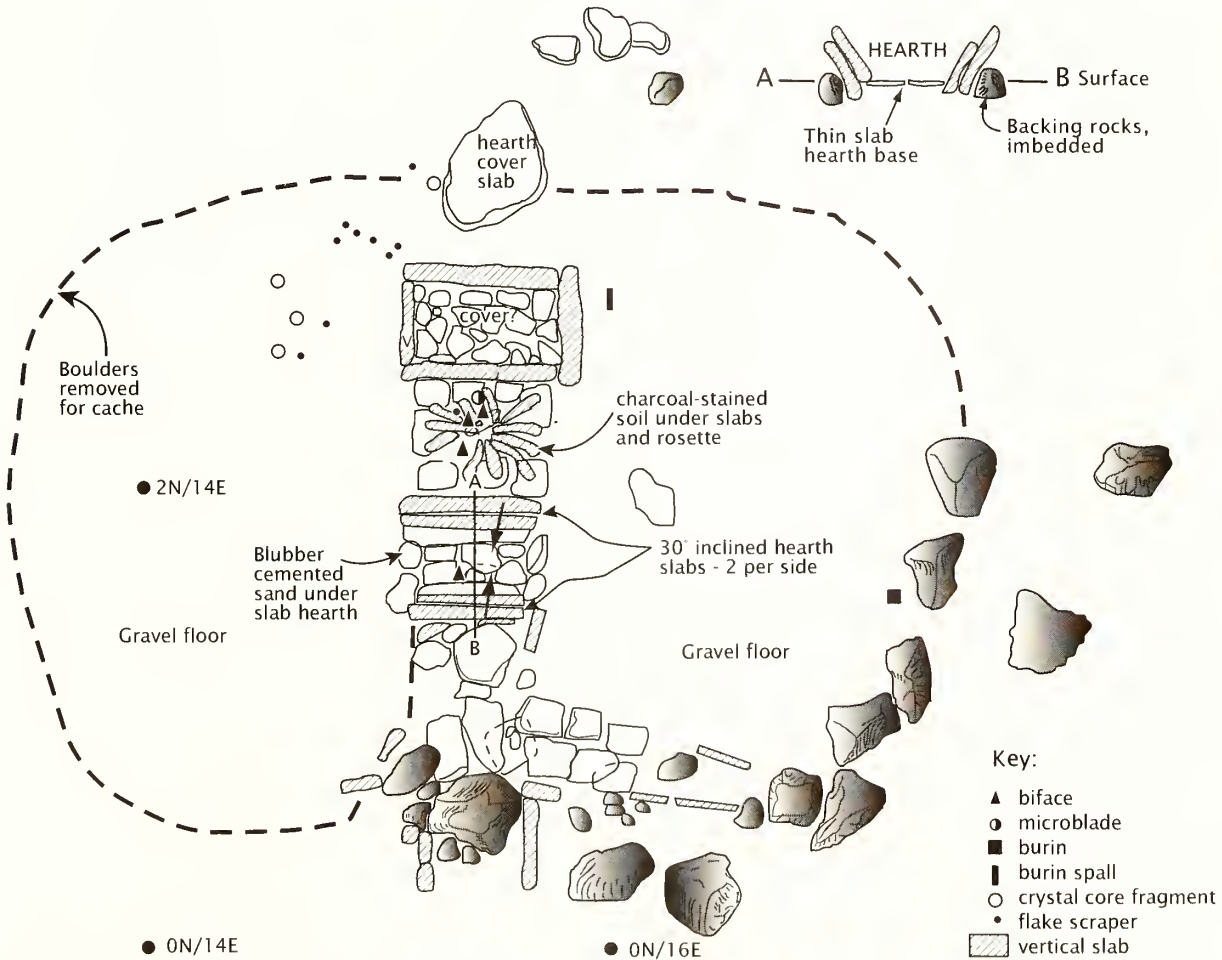
Groswater although it displays movement in this direction. Its placement as a Terminal Pre-Dorset phase, therefore, seems warranted on both dating and typological grounds.

Spatial Patterns

The preservation of the Nuk-2 architecture provides some insights into social behavior. As noted previously, the sleeping areas produced only a few scattered tools and no debitage and must have been covered with bedding. Lithics were primarily recovered from the hearth complex and adjacent regions, near pavements, and in the foyer and workshop or midden areas immediately downslope from the entry of S2.

In S1, other than a single burin from near the wall of the east room, all artifacts were found in the vicinity of

the hearths and the northwest entry pavement (fig. 13.13). A small Ramah chert side-notched knife blade was found in Hearth 1, and two biface tips, a biface midsection, a utilized flake, and a microblade were recovered from the conical hearth pit in Hearth 2. Five quartz crystal cores were found with the only concentration of debitage in the dwelling west of the box hearth. No external middens or workshop areas were noted. These patterns suggest that the hearths, especially the pit hearth, were used as work and maintenance areas and that microblade and lithic tool production took place in the entry area. The most significant feature of this distribution, however, is the occurrence of most tools in the axial hearth and the overall paucity of artifacts and debitage, suggesting a relatively brief occupation and limited lithic supplies.

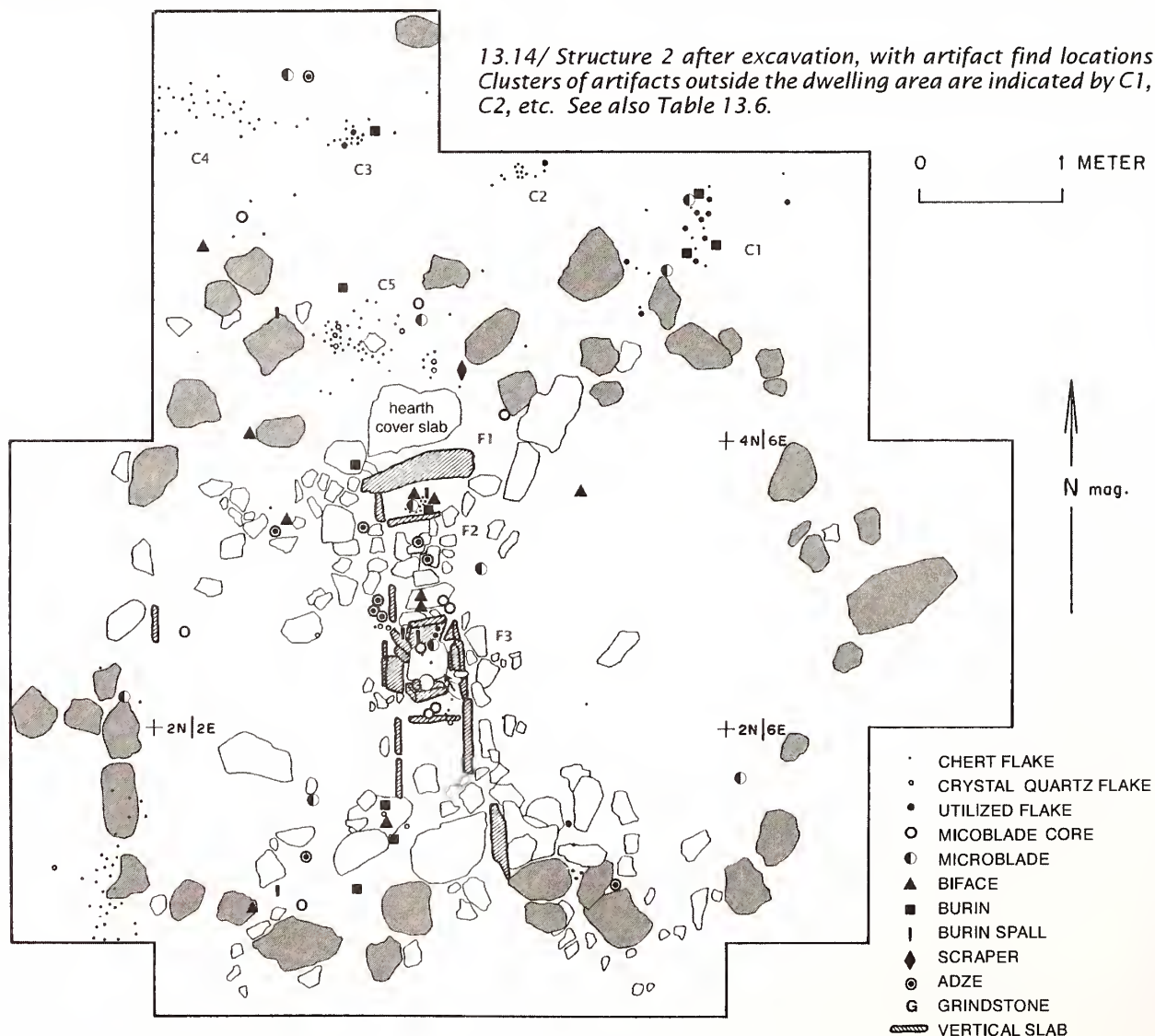


13.13/ Structure 1 after excavation with artifact distribution

Structure 2 was more productive in terms of artifacts and produced 61 diagnostic specimens (fig. 13.14). Again, the dominant pattern was the segregation of finds between areas that are presumed to have been covered or left open when in use. Only six implements were found in the gravel (presumably skin-covered) lateral compartments, whereas more than fifty implements were found in or immediately adjacent to the hearth, pavement, and midden areas. The spatial distribution of artifacts is provided in Table 13.6. A few artifacts and flakes were also recovered in a small external midden or work area.

Lateral sleeping areas have few finds and seem to have been areas of limited lithic tool production or discard. The west room contained a quartz core, two

microblades, a celt, two bifaces, and an interesting notched microblade with a polished tip for hide working (fig. 13.8f), possibly as a boot-creaser. The east room contained a biface fragment and two microblades. A greater concentration of finds occurs in the uncovered areas of the dwelling. The east wing pavement contained a broken celt bit (fig. 13.9e) and a utilized flake. The west wing pavement had three ground burins (fig. 13.10f-h), a burin spall, a celt with a broken poll end (fig. 13.9c), two biface fragments (fig. 13.8b, 13.8e), and a quartz core. Hearth F3 contained sixteen tools: crystal cores, microblades, burin spalls, and points (fig. 13.11 a, b). Six celts were found in or near Hearth F2, including one complete celt (fig. 13.9a), a celt poll fragment (fig. 13.9b), and three celt blanks



(fig. 13.9d, e, f); these would have been used in chopping wood, bone, or frozen meat. The “boiler box” (F1) contained two biface fragments (fig. 13.8c, d), a burin spall, two burins (fig. 13.10e, i), and a utilized flake. The foyer collection included a variety of production and maintenance tools in a thin scatter—a scraper, a burin-like tool (fig. 13.10c), microblades, and burin spalls—while in the foyer and outside the dwelling along the north wall five concentrations of tools and debitage were found that might be interpreted either as middens or as activity areas (fig. 13.14 C1–C5). Cluster 1 (eastern midden) produced four burin-like tools (fig. 13.10a, b, d, j), microblades, and flake tools; Cluster 2, a utilized flake; Cluster 3, a burin-like tool tip (fig. 13.10j), and two utilized flakes; Cluster 4, a microblade and adze fragment; and Cluster 5 (inside the foyer), the densest concentration of debitage, produced Mugford chert and quartz crystal flakes, a burin-like tool and spall, an endscraper, crystal cores, and a microblade.

As in S1, the distribution of these finds emphasizes the importance of activities in the hearth and pavement areas within the structure and in the space in front of the dwelling. However, these areas do not exhibit much evidence for the segregation of specific activities, as most tool types are distributed evenly between internal paved areas, the axial hearth, and the exterior. It is interesting to note that formal scrapers are almost absent from the entire Nuk-2 assemblage. On the other hand, the large number of celts, both broken and complete, and celt blanks suggests that heavy-duty cutting and chopping (butchering frozen meat?) were important activities. Most of the celts were recovered at the north end of the hearth area where a cache of celt blanks was found. Celts were also found on the wing pavements. Small Ramah chert cutting knives and a notched microblade knife were associated primarily with Hearth F3, and crystal microblade cores were found here and along the front wall near the foyer.

Table 13.6/ Distribution of Structure 2 finds by area

Area	Artifact Count	Artifact Types
East room	4	1 bif, 2mb, 1c, 1uf
West room	5	1C, 2bif, 1mb, 1c
<i>subtotal</i>	<i>(9)</i>	
Stone box	6	2blt, 1bs, 2bif, 1mb
Hearth A	10	6C, 2bif, 2c
Hearth B	9	2bs, 1mb, 4c, 2uf
<i>subtotal</i>	<i>(25)</i>	
Foyer/Entry [C5]	6	1blt, 1bs, 1mb, 2c, 1s
East wing	2	1C, 1uf
West wing	8	3blt, 1bs, 1C, 2bif, 1mb
<i>subtotal</i>	<i>(16)</i>	
East midden [C1, 2]	16	3blt, 2mb, 11uf
West midden [C3, 4]	6	1blt, 1bif, 1mb, 1c, 2uf
<i>subtotal</i>	<i>(22)</i>	
Total	72	

Key: blt=burin-like tool, bs=burin spall, C=celt, bif=biface, mb=microblade, c=core, s=scraper, uf=utilized flake

From this distribution one may deduce that: (1) the working of bone tools with burins and celts seems to have occurred on the rear wing pavements; (2) food processing and maintenance activities, using celts and small knives, are associated with the axial hearths; (3) lithic tool maintenance was conducted almost exclusively in the foyer and outside the dwelling proper, in five areas where debitage, burins, burin blanks, and crystal cores predominate. Lacking bone preservation, it is impossible to determine whether these areas are midden dumps or work stations, but the latter appears more likely given the presence of unfinished implements and the absence of lithic debris inside the house; and (4) most lithic production and use occurred away from the sleeping areas. Overall, the light and heat of the hearth seem to have created the nexus for social and production activities within the house for the axial feature contained the majority of the formal tool finds.

Seasonality

Knuth (1967:32,52) and many others since then have cautioned about using faunal remains to determine seasonality in situations, as commonly found in the Arctic, in which meat killed in one season was cached and eaten in another season. For that reason, Knuth relied more on the presence of cache structures and wall rocks to indicate winter settlement. Ramsden and Murray (1994) suggest the opposite, assigning

isolated hearth structures to winter use and rock-ringed axial structures to summer use based on the presence of winter game (caribou) or summer game (ducks), and local geographic factors. Bielawski (1988) also discusses the problem of determining seasonality in Early Paleoeskimo archaeology. Lacking faunal remains, Nuk-2 seasonality must be approached from other directions:

1. As noted previously, the location of the Nuk-2 houses near the top of an exposed beach series, high above and distant from the shore, without a view and in a nearly "hidden" setting, is without precedent as a summer or open-water location for coastal Paleoeskimo or Neoeskimo sites currently known in Labrador. Occasionally, Dorset and Neoeskimo winter sites have been found in such settings.
2. Given the lack of formal architectural features in many Labrador Paleoeskimo sites, Nuk-2 stands out as having been carefully planned and constructed. Such constructions are unknown for the many small, shore-side Paleoeskimo sites found in Labrador that are known or can be inferred to have been occupied in spring and summer. The care that went into planning the Nuk-2 structures indicates attention to details that are more consistent with the construction of sedentary, cold season dwellings than with more transient warm season dwellings.
3. Celts have been shown to be an indicator of winter seasonality in Labrador Paleoeskimo sites. These tools are rarely found in the shoreside Pre-Dorset or Groswater sites that are abundant in the treeless mid/outer coast regions. Celts are found at Paleoeskimo sites in forested inner-bay regions, such as at the Postville Groswater site and in Pre-Dorset sites in the Port Manvers Run region of Nain, where wood and caribou provide favorable conditions for Early Paleoeskimo fall and early winter settlement. Postville, which contained thick deposits of fire-cracked slabs, burned rocks, charcoal, and slab hearths produced many celts and celt fragments. The presence of celts as indicators of cold season settlement is even more evident in the Late Paleoeskimo tradition when celts are made of nephrite, a rare material that is carefully curated and is rarely found in Dorset summer season camp deposits. Celts, however, are

found frequently in Dorset cold season sod-house sites and middens because of the increased need during the winter months for butchering frozen meat, chopping firewood, and production tasks involving roughing out ivory, bone, and wood implements. Apart from archaeological evidence, in arctic ethnographic cultures these activities are conducted primarily between October and May.

4. Analogy with Middle Dorset sites, where there is a clear differentiation between winter sod-house dwellings and open-water season tent dwellings with slab pavements and open hearths, suggests seasonal patterning applicable to Early Paleoeskimo settlement models. In Middle Dorset spring or open-water season camps, lithic distributions usually occur in circular patterns around central hearths that are interpreted as floor deposits in tent enclosures. In many cases, these distributions are enclosed within tent rings, but in others they may be open-air sites. In either case, these scatters often seem to fall on uncovered ground, whereas in winter dwellings most internal space is covered with bedding that restricts lithic accumulations to specific exposed hearth or work areas. Following this analogy, the distribution of Nuk-2 finds in the axial hearth area suggests a fall, winter, or spring season occupation.

5. Nuk-2 can be compared with the axial structures of the Pre-Dorset, Groswater, Early Dorset, and Late Dorset sites whose formalized, "heavy" construction features and presence of fire-cracked rock and encrusted blubber are indicative of cold season occupation. The case is strongest with the Okak 3, Peabody Point, and Big Head Late Dorset sites. The latter follow the architectural pattern of the Nuk-2 houses, having carefully constructed axial features with border rocks, hearths, transverse dividers, pavements, and other features. Heavily encrusted lamp stands and the presence of thick charcoal deposits, extensive cultural deposits, and partially excavated floors all suggest late fall/early winter occupations of these Late Dorset structures, probably from September through December. A comparable period of occupation seems reasonable for Nuk-2.

6. Finally, Nuk-2 is located near the fall harp seal migration route and would have been well-positioned to take advantage of this important resource.

This said, one wonders—if this was a full, winter-long habitation—why the paucity of artifacts, especially from Structure 1, and the small quantity of debitage recovered; the lack of nearby food caches; and the presence of outdoor lithic maintenance activity? None of these points necessarily require summer seasonality. The inception of dwelling construction in the winter would have been difficult because of frozen ground. All things considered, the evidence from Nuk-2 points toward its construction and occupation from October through December.

Settlement Patterns

Given the paucity of information from Nuk-2 and the few other Late or Terminal Pre-Dorset sites in Labrador, it is impossible to reconstruct a precise settlement pattern at this time. Nuk-2 establishes only one seasonal point in this system, and it appears atypical since no other outer-island winter EPE sites are known, even after intensive survey coverage of these habitats. Nor is Nuk-2 typical of the preceding EPE tradition, for despite large numbers of Pre-Dorset sites from the northern Labrador coast, none except Nuk-2 can be identified as a winter settlement. The typical EPD pattern in the Nain area is for outer-coast settlement in spring and summer and inner-bay (Port Manvers Run, especially) settlement during fall; winter sites have never been found on the coast. This leads us to believe that Early Pre-Dorset people in northern Labrador moved into the interior for the winter as they did in Peary Land, where they hunted musk ox and fished for char (Knuth 1967); in the Central Arctic, Pre-Dorset groups are also thought to have utilized terrestrial as well as marine resources during the winter (Maxwell 1985:88–90). In Labrador, caribou and char on near-interior lakes are the most probable winter quarry, and in this region the existence of the forest would have been a major attraction for housing and heat, especially as the EPE people lacked large blubber lamps and soapstone cooking vessels. It seems likely that a similar pattern would have held for LPD. If this is true, the Nuk-2 site,

which I believe was a winter site, is anomalous, not only for the Pre-Dorset but also, given our current knowledge based on the Postville Groswater site (located in the forested inner portion of Kaipokak Bay), for the subsequent Transitional Groswater phase.

In other ways, however, Nuk-2 conforms to the known EPE pattern of small settlement size and dispersed demography, in this case as an encampment of one or two tents, each occupied, judging from the small sites of the structure, by six to eight individuals. The small quantity of tools and debitage suggests that the site was occupied for only one season, and probably for only a few months. Both structures were likely occupied at the same time, although S1 may have been abandoned before S2, accounting for the cannibalization of its wall rocks, cache construction, and its smaller artifact and flake inventory. Middle and Late Dorset winter camps also frequently consist of two contemporary dwellings. Nukasusutok and nearby islands were favored areas for Dorset winter settlements in later periods, when two-house communities were also the norm, although of larger size. But these sites are all situated close to the water and not at high locations hundreds of meters from the shore.

Nuk-2, therefore, appears unusual from a number of perspectives. Its topographic position is unique among other Paleoeskimo sites known in Labrador; and its suggested fall-winter seasonality and outer-island location does not fit the typical EPE settlement pattern. Furthermore, it is the southernmost LPD site currently known in Labrador and seems to have been occupied for a very brief period and lacked ample supplies of lithic raw material. One wonders why Structure 1 was partially dismantled, with a cache constructed on its hearth pavement, while Structure 2 was abandoned with its structure virtually intact. However, in other respects, Nuk-2 is typical of several other EPE dwelling sites excavated in northern Labrador with peripheral tent rings, axial pavements with hearth features, brief occupation durations, and no extensive middens, in addition to low debitage

and artifact returns, the presence of celts, a high frequency of burins, a low frequency of microblades, and a concentration of finds primarily in the axial pavement areas. The extremely frugal nature of lithic debitage, all of which represents maintenance rather than manufacturing activities, suggests that the people at Nuk-2 had very few lithic tools and little lithic raw material to spare. A knowledge of the Ramah and Mugford chert sources is evident, but tool conservation and the sparse use of lithics (except for the prolific wastage of locally obtained celt materials) are consistent with what one would expect of a pioneering occupation by a small group with very limited regional infrastructure.

One of the intriguing problems emerging from our Labrador surveys also is highlighted by the unusual preservation of the Nuk-2 site. Standing box hearths are rare in Labrador and have been found elsewhere only as isolated features without axial pavements (e.g., at Karl Oom and Nulliak). Considering the large number of EPE sites recorded in the region from Nain to Saglek, the scarcity of box hearths is peculiar. The same may be said of formal axial dwelling constructions. The rarity of these site types suggests that their absence may result from the lack of preservation due to post-occupational processes or from the possibility that such sites, presumably of winter seasonality, are located primarily in inner-bay and interior regions, which are still poorly known archaeologically. At present, the lack of survey data is probably the most likely cause for the ambiguity in our current understanding of EPE and Transitional settlement patterns.

This tentative reconstruction suggests that Labrador EPE people may have had a settlement cycle that involved shifting between summer sites on the coast and fall and winter sites on the forested inner bays and interior, where caribou, char, and wood to heat their poorly insulated winter tents were abundant. A similar summer coastal and winter interior pattern has been suggested for early Paleoeskimo sites in Peary Land. Today's Inuit of Nain tell of their

ancestors shifting from coastal sites into the forested interior, to the lakes west of the Kiglapaits where they could be sure to find fish, in rare instances when they were faced with starving in winter on the coast (Abel Leo, personal communication 1976). An absence of caches, lack of fire-cracked rock deposits, and an orientation to open-water hunting zones are significant factors indicating summer seasonality of EPE site distribution in coastal Labrador. A full reconstruction of this settlement system requires an additional winter component of the type represented by the Groswater Pentacostal site in Postville, with its cluster of house floors, thick black-earth deposits, large artifact and debitage collections, and layers of fire-cracked slabs and hearth rocks. To date, no Pre-Dorset winter sites have been located in Labrador, but it seems likely that they will eventually be found on the near-interior lakes and caribou hunting grounds. In all of these respects, however, Nuk-2 remains anomalous as a fall-winter site located near the ice-floe edge.

Terminal Pre-Dorset

The foregoing discussion demonstrates that the Nuk-2 complex has many technological similarities with late EPE complexes known elsewhere in the Eastern Arctic. Nuk-2 burins postdate Taylor's Arnapiik material, which he estimated at 3000–3500 B.P. based on burin styles, but they are similar stylistically to burins at Nagy's (and Taylor's) Pita site dated at 2580 ± 60 B.P., although it may date somewhat earlier (Nagy 2000b:35). Nuk-2 is certainly later than McGhee's Gull Cliff Port Refuge site dated at 3140 ± 55 B.P. and 3505 ± 55 B.P., but which McGhee (1981:123) believes should date earlier (ca. 3500–3700 B.P.) based on the absence of polished or notched burins. For similar reasons—the presence of waisting, notching, small size, and polishing features of burin technology—Nuk-2 should postdate the Twin Ponds Complex (3500–3700 B.P.; Helmer 1991:309); Bloody Falls (3300 ± 90 B.P.; McGhee 1970); Umingmak (3400–3300 B.P.; Müller-Beck 1977), and the Sarqaq and

Pre-Dorset phases of Bache Peninsula and Buchanan Bay (Schledermann 1990, 1996). On the other hand, the Nuk-2 complex shares a close relationship with Helmer's Devon Lowlands Rocky Point Complex (3000–2800 B.P.), which he dates by comparison to Nuk-2 and Okak 4 (Helmer 1991:313). There seems to be no exact parallel to the Nuk-2 assemblage in Schledermann's Buchanan-Bache sequence.

It is difficult at this point to present a clear distinction between Terminal and Late Pre-Dorset because of the small sample of sites available for this period in Labrador. Many of the features Cox (1978:104) described for Late Pre-Dorset lithics are also present at Nuk-2: spalled burins with ground faces and tips; larger (than EPE) triangular endblades with both flat and concave bases; large stemmed endblades, small endblade knives, tabular end scrapers, and limited microblade production; and a tendency for waisting, shallow side-notching, and single-double asymmetric notching in hafting styles. Edge serration is only an occasional rather than a regular feature of biface production, as in earlier periods. Soapstone lamp fragments are rare but present. When present with late-style burins, Nuk-2 houses, and certain Groswater features (flat adzes, notched points, etc.), the differences from Late Pre-Dorset (3500–3200 B.P.) may be sufficient to warrant a separate Terminal Pre-Dorset phase dating to 3200–2800 B.P. Alternatively, as more evidence of this period becomes available, it may suggest that TPD should be classified with Groswater and Independence II as components of the Transitional Horizon.

"Grey Culture" Distress?

On a more abstract level, while it is identifiable as a chronological horizon, the TPD phase lacks the strong identity characteristic of earlier EPE and later Groswater or Transitional PE complexes. Not only in Labrador but also elsewhere in the eastern arctic, TPD sites and collections tend to be small and of variable technology, suggesting that this period was one

of low population density, short-term settlement, and weakly expressed cultural patterning (Helmer 1991:315; McGhee 1979:118; Schledermann 1990). In contrast to earlier and later periods in which assemblages are dominated by an abundance of fine cherts and carefully made, consistently styled tools, TPD assemblages are often made from low-quality materials like vein quartz and poor-quality local cherts. Such characteristics suggest restricted demographic mobility and limited access to high-quality lithic materials that were more abundant in earlier Pre-Dorset and later Groswater periods. Tool size and debitage production are reduced from earlier periods, reflecting a scarcity of lithic stock, and there is evidence of intense curation of individual tools. These patterns suggest a breakdown of regional exchange systems and a shortage of high-quality lithic materials, which is one of the primary hallmarks of earlier and later period technology. When coupled with indications of small dispersed sites and low population levels, one suspects that LPE culture was enduring unusual stress and disorganization.

A comparable episode in Eastern Arctic history may be seen in the Protohistoric period between A.D. 1500 and 1850. During this time, many Inuit groups faced environmental, social, technological, and perhaps biological challenges stemming from a combination of climatic and environmental change and external contacts resulting from new European visitors. The loss of whaling potential, shifts and perhaps declines in caribou and musk ox, the introduction of new materials and diseases, territorial shifts, and confrontations with dwindling Dorset populations and Indian groups may have provoked physical, biological, and spiritual stress. Such stress appears to have disrupted systems of regional interaction and exchange, undermined ritual and social systems, and produced disarray in technological templates, resulting in a proliferation of regional styles and ad hoc technology (McGhee 1972b:129). Disarticulated cultural systems seem evident in the protohistoric Copper Eskimo area (McGhee 1971, 1972b), and low population densities are suggested

for seventeenth-century Labrador (Kaplan 1983:326). The transition from Classic Thule to its Developed and Historic phases is also accompanied by environmental stress resulting from the onset of the Little Ice Age, the loss of central arctic whaling, and subsistence-settlement reorganization. Throughout the Central Arctic, an archaeological hiatus is evident in the abandonment of former habitation areas, indications of population decline, and absence of strongly patterned cultural remains. Compared with Early Pre-Dorset, Transitional Paleoeskimo, and Thule—all of which have strong cultural profiles—Late Pre-Dorset culture, in general, and Terminal Pre-Dorset, in particular, appear to be examples of those near-invisible “grey” cultures that herald change or exist at times of cultural transition. Such a model may help interpret what appears to be a period of low cultural profile in Labrador Paleoeskimo history.

One wonders why the Late/Transitional Pre-Dorset phase in Labrador might have fewer sites and lower populations than the previous EPD phase. One possible explanation may relate to the fact that the period from 3500 to 3000 B.P. is marked by a northern expansion of Indian culture (Saunders phase), reaching Okak and Hebron, with occasional forays for Ramah chert and caribou hunting north as far as Saglek and Ramah. An Indian presence in these regions could have restricted or endangered Pre-Dorset access to forest resources, animals, or preferred lithic raw materials like the grey and black cherts of the Mugford/Kaumajet region north of Okak. The loss of these territories and products would have impacted Paleoeskimo technology and belief systems. Among hunting cultures, lithics used for killing and butchering animals and their geological sources are closely connected to hunting ritual and human-animal spirit exchange (Loring this volume). Source localities of these materials are often held sacred and are occupied by powerful deities. The loss of these areas to alien groups would have resulted in social, spiritual, and technological distress (Fitzhugh 1984b).

Cultural stagnation, abandonment of previously populated regions, and a northern advance of Indian populations, if not the causes of LPD/TPD retrenchment and decline, would have been major problems for Paleoeskimo groups in Labrador immediately preceding the Nuk-2 occupation. In addition to difficulties in obtaining high-quality chert and the loss of important hunting territories in the northern forest fringe, Pre-Dorset peoples in Labrador and further north would have lost access to useful forest products. The extent to which such upheavals may have influenced depopulation and cultural decline elsewhere in the Eastern Arctic is unknown. One imaginable and likely event that could have triggered broad-scale changes lies in the area of ungulate biology and climate. The period following 3500 B.P. was also one of climatic change, including cooling and unstable climatic events (Majewski and Bender 1995). Forest boundaries west of Hudson Bay retreated in a series of dramatic forest fires (Nichols 1967), and similar events occurred in Labrador-Quebec, though with less pronounced effect on the northern forest limit (Fitzhugh and Lamb 1985:363; Short 1978; Short and Nichols 1977). The disruption of caribou herds would have had a profound effect on EPE winter economy, as would the disruption caused by hostilities and resource loss resulting from encroachment by Indian groups.

In this respect, the Nuk-2 occupation appears to be a “terminal” EPE horizon in Labrador; its 3000 B.P. date falls shortly before the appearance of the Groswater phase, which begins in Labrador ca. 2800 B.P. and slightly later, ca. 2500 B.P., on the Lower North Shore and Newfoundland (Fitzhugh 1980b; Loring and Cox 1986; Pintal 1994; Renouf 1994). Given Nuk-2’s unique setting as a probable fall or early winter season site on an outer island, its lack of obvious economic orientation, and its spartan lithic usage, the site might have been an “exploratory” venture to recolonize territory formerly occupied by previous Early Paleoeskimo peoples between 3400 and 3000 B.P. that had been taken over by Saunders Complex Indian groups. The

high “hidden” aspect of the site and its outer-coast orientation offers support for such an hypothesis. If so, the effort appears to have failed. This complex did not lead to an expanded TPD occupation in central Labrador, and it is not a likely progenitor of the succeeding Groswater Phase.

Relationship to Transitional Horizon Cultures

Links to the succeeding Groswater phase need to be discussed. As noted previously, Groswater appears in Labrador as a fully formed cultural entity with tightly defined typological templates and significantly different technology. With a considerable body of Groswater data now available from Labrador, Newfoundland, and the Lower North Shore, there appears to be little evidence of stylistic change throughout its 600- to 800-year duration. Complexes of 2800–2600 B.P. are nearly identical to those of 2200 B.P. The period of major change was between 3000 and 2800 B.P.

Although not immediately ancestral, the roots of Groswater are apparent in Transitional Pre-Dorset components like those at Nuk-2: ground burins, incipient side-notching, partially ground flat slab celts, absence of nephrite, restricted use of soapstone cooking vessels, and similar dwelling types and adaptations. But equally significant are the differences: Groswater’s extensive microblade technology, deeply side-notched and box-based plano-convex harpoon endblades, wide-eared end scrapers, and ground Sarqaq-like boot-creasers. It is not likely that these Groswater elements evolved directly from a Labrador Nuk-2 prototype, especially given the marginal nature of this occupation in Nain and northern Labrador. These changes more likely occurred in TPD societies farther north. The presence of highly styled Groswater phase bifaces at Port au Choix East and West (Renouf 1994), together with other elements not present in Labrador Groswater sites, leads one to conclude that they originated in Newfoundland as a specialized development from a more generalized Labrador Groswater culture. Nevertheless, the possibility of a Newfoundland origin of Groswater

innovations should not be dismissed, for no early Groswater site has yet been found in Labrador that lacks the use of distinctive mottled southwestern Newfoundland cherts, which is what one would expect to see in a pioneering group that had not yet reached Newfoundland. Such a complex may be represented at the Big Falls site in Saglek (Tuck 1975b), whose undated assemblage is Groswater but consists of Ramah rather than Newfoundland cherts; in this case, however, the site is sitting virtually on top of a Ramah chert outcrop, so that its raw material content may be irrelevant. Northern origins may also be indicated by early 3000 B.P. dates on willow samples from Independence II sites in Peary Land and by the larger numbers of Transitional Period sites known in northern Quebec. Whatever kicked Groswater and the Eastern Arctic Transitional cultures “into gear” remains undisclosed. Yet, the evidence to date suggests that the southeast subarctic region became a distinct subregion of the larger Eastern Arctic Transitional Horizon.

There seems to be a continuing role for southern connections in early forms of Dorset, although in this case it may be in the form of raw materials rather than slate tools, side-notching, or concave scrapers as originally proposed by Collins (1962). Southern chert, among other boreal resources, may have been part of the incentive for the remarkably rapid southern thrust of early Groswater into central Labrador, Newfoundland, and Quebec. So strong is the role of Newfoundland chert in the earliest Groswater assemblages in Labrador that one suspects a Terminal Pre-Dorset complex may someday be found on the island. If so, Collins’ and Meldgaard’s intuition about the “forest smell” of Dorset may yet be revived.

With the immediate link between Nuk-2 and the succeeding Groswater period not yet evident in Labrador, we must look farther north for origins, since pre-Groswater Paleoeskimo sites have not been identified in Newfoundland. But so far, northern sites have not produced convincing links between the LPD/TPD and Transitional phases. Late Paleoeskimo sites do not

contain architectural evidence comparable to Nuk-2, and these assemblages are not obvious transitional prototypes. To cite one of the complexities, the Nuk-2 assemblage is most similar to LPD-phase lithics but its dwelling types are closest to Transitional Horizon Independence II, although the latter sites lack stone boiling boxes. Thus, it may be that Nuk-2 house ancestry predates Independence II houses (Knuth 1981:fig. 9a) by a few hundred years and that stone box cooking, which probably evolved from the Independence I/EPD quadrilateral hearth feature, was discontinued in northern Greenland before the Transitional Horizon coalesces about 3000 B.P. Under this scenario, Nuk-2 architecture would have been derived from an as-yet-undiscovered Sarqaq or Independence I successor in the Eastern Canadian Arctic.

Since Alaskan contact has often been cited as a stimulus for Dorset origins, we may expect sites from the Western Canadian Arctic to hold clues, but here again we are faced with questions rather than solutions. Western traits are present in the Lagoon complex, but its Crane and Lagoon components (2600 B.P. and 2300 B.P., respectively) are too late to be progenitors of Independence II. Nevertheless, the strong Dorset "cast" of this material and the presence of both Alaskan and Central Arctic elements offer continued support for the hypothesis of western influence, especially in semisubterranean house forms, notched implements, cooking vessels, and burin technology. Significantly, these innovations, which appear in Early (T1) Dorset, seem to have had little, if any, influence on the origins of the Transitional Horizon Independence II and Groswater complexes. The latter appear wholly original to the eastern region.

Early Dorset and the Late Paleoeskimo Tradition

The final stage of this transformation, the origins of the Late Paleoeskimo tradition and its first culture phase, Early Dorset, appears more closely connected to the Lagoon Complex than to the preceding Transitional

Horizon. The best definition for Early Dorset still is Collins's T1 site assemblage, which displays all of the new features that distinguish this subtradition from its EPE predecessor. These include the presence of fully ground (after spalling) burins, concave base plano-convex tip-fluted endblades, and rectangular soapstone cooking vessels in place of stone boiling; more sedentary winter coastal adaptation with shallow semisubterranean dwellings; a proliferation of ground-slate endblades; and new forms of celts and toggling harpoons. These features are not found in the earlier Transitional complexes of the Eastern Arctic. Yet because they appear in sites in the Central and Western Canadian Arctic rather than in Greenland, Newfoundland, or Labrador, and because they overlap chronologically with Transitional complexes of the latter regions, the nature of culture change as an expanding, eastward-moving wave of migration, stimulus, or acculturation seems evident.

This dynamic process is most apparent in the Eastern Arctic and Subarctic during the EPE-LPE interface. Occurring within the span of only 500 years, between 3000 and 2500 B.P., Early Paleoeskimo traditions reconfigure into a distinctively new culture known as Independence II in northern regions and Groswater in the south. Yet, hardly had these changes taken place before a new complex with significantly different technological, adaptation, and settlement forms appeared. Since Early Dorset innovations cannot be derived from Transitional Horizon cultures, it is tempting to see them as having western, perhaps even Siberian, origins. Marine rather than land resources became dominant, and the focus of activity shifted toward permanent coastal residence. Seals and walrus replaced a mixed land/sea (caribou/musk ox/seal) focus for winter food; artistic expression elaborated; and shamanism became a recognizable feature of archaeological collections. Once implanted, the Dorset tradition spread rapidly beyond the Central Arctic core area, as Transitional cultures seem to have done only a few centuries earlier. In most areas, Early Dorset replaced or absorbed Transitional

cultures without leaving many traces of the former tradition. Only in Newfoundland does it appear that Groswater lithic elements were incorporated into the Dorset tradition.

The speed with which these changes took place, over great distances, is a remarkable aspect of the Paleoeskimo transition. These responses, which must have been influenced by the cooling episode of the sub-Atlantic, also mark the cultures of this period as highly dynamic and creative. Developing new economies and tool forms; forging new alliances and routes for the acquisition of distant trade materials; and dealing with social and perhaps even ethnic and linguistic divisions—not only in Labrador and Newfoundland but within the Central Arctic regions and Greenland—Dorset Paleoeskimo peoples demonstrated a remarkable capacity for dynamism and change. From this perspective, their history during this period cannot be subsumed under a rubric of stasis or stability. This time marked the greatest period of change to occur between the first arrival of Paleoeskimo peoples and the Thule culture's appearance 2,000 years later.

Finally, this review of the Transitional Horizon concludes with an observation on terminology. Nuk-2 provides evidence for cultural continuity within the EPE tradition from Early Pre-Dorset into the Transitional Horizon based on technology, typology, settlement patterns, adaptations, and dwelling types. The new elements that appear in Groswater and Independence II are primarily stylistic and internally generated. Labrador TPD/GW responded to new ideas like lamps and ground burins in their own conservative way; however, this adaption was eventually replaced by a more successful one originating in the Central Arctic. Seen in this light, the LPE tradition represents a departure from the previous EPD tradition. For this reason it seems appropriate to classify the T1 type of Early Dorset as the beginning of a new subtradition and to place Groswater and Independence II as regional variants of Transitional Horizon cultures that still retain the basic organization of the preceding Early Paleoeskimo

tradition. While there is merit in the concept of a "Meso-eskimo" tradition as first outlined by Knuth and elaborated by Plumet, continuities between Early and Late Paleoeskimo still seem strong enough to support the idea of a single Paleoeskimo tradition, a conclusion also reached by Nagy (2000b:114). In the future, if more specific Dorset origins should be identified in Alaska and ethnic boundaries such as those suspected between Groswater and Early Dorset in Labrador were to be seen as more widespread in the Eastern Arctic, perhaps "Dorset" rather than "Mesoeskimo" would be preferable as a name for a third subtradition within the broader Paleoeskimo period.

Nuk-2 opens a window to a new and dynamic period in the history of Labrador cultures. It not only offers a glimpse of a unique pioneering occupation by Paleoeskimo peoples probing the southern frontier of the arctic world, but it also raises questions about events that were to follow as a rapid succession of northern cultures adapted arctic lifeways to subarctic Labrador and spread even farther south, into Newfoundland and Quebec's Lower North Shore. Doubtless, there will be other surprises in store as Labrador's past continues to be uncovered.

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To me, the discovery of the Nuk-2 site in 1976 came like a bolt from northern Greenland, but when I told Eigil Knuth about Independence II houses in Labrador, it came to him as an equally dramatic shockwave from the boreal forest. I owe much to Eigil Knuth for the inspiration of his pioneering work in Peary Land and to William E. Taylor and James V. Wright for encouraging me to undertake work in Labrador. But it is to Elmer Harp that I owe my deepest gratitude for training, inspiration, and opportunities that come only once in a lifetime. This paper reflects years of discussion with many individuals, primarily Steven Cox, Richard Jordan, Stephen Loring, and Christopher Nagle, and more recently, Daniel Odess, none of whom should be held responsible for any shortcomings. I especially wish to

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Notes

1. Radiocarbon dates listed as B.C./A.D. have been calibrated using the Stuiver and Pearson 1987, Rev. 1.3 conversion program. Otherwise, dates or periods listed as B.P. are based on uncorrected C14 ages. Some of the latter have been corrected for C12/13.

“And They Took Away the Stones from Ramah”:¹ Lithic Raw Material Sourcing and Eastern Arctic Archaeology

STEPHEN LORING

The quickening pace of archaeological research throughout the North American Arctic in recent years has destroyed many of the comfortable, uncomplicated views formerly held about the development of prehistoric cultures there.
(Harp 1964b:184)

It is hard to imagine an archaeology without stone tools. Stone tools figure prominently in the definition of the human species, and as traces of past cultural presence their record is of the greatest duration and the broadest spatial distribution in defining our global tenure. The permanence of stone tools has been an inspiration for archaeologists and essayists alike (e.g., Thoreau 1962 [1906]:1212, 1454–1455).

Like many of their lower-latitude brethren, arctic archaeologists have relied disproportionately on stone tools, especially projectile points and bifaces, when erecting their interpretations of the past, disproportionately so in the sense that stone forms such a small percentage of the raw materials used by ancient human groups whose skin, wood, and bone artifact industries have often not survived. While arctic archaeologists are sometimes graced with frozen middens that can provide insight into the ancient perishable assemblages of former arctic foragers, stone yet remains the material culture currency of favor, and more so the further back in time one goes.

Prehistoric arctic inhabitants were an extraordinarily resourceful lot. Their knowledge of the intricacies and nuances of their mostly frozen world are, for the most part, beyond the ken, even beyond the imagination, of most people today. In a world where survival



placed a premium on ingenuity and on knowledge derived from wide-ranging movement across the landscape, it is not surprising that what little the land afforded in the way of mineral resources was discovered and utilized. Across the Arctic, Inuit ancestors and their predecessors had discovered much that was of interest to them, including fossilized Pleistocene bones, meteoritic iron, float copper, coal, steatite, amber, nephrite, slate, quartz, crystal quartz, and, of course, a wide variety of cryptocrystalline silicates, fine-grained cherts, obsidian and metamorphosed sediments. These materials were used to fabricate the myriad hunting, butchering and manufacturing tools on which life was contingent and must have played a central role in the economic and spiritual life of these people.

The conchoidal fracturing properties of chert (the general term that geologists use to refer to sedimentary rocks composed of cryptocrystalline silicas, including materials also called flint, jasper, chalcedony, novaculite, agate, and quartzite) by which stone could be manipulated to produce a wide variety of cutting, scraping, and piercing edges, made knowledge of the

sources and varieties of this material a critical component of ancient Inuit adaptations. Useable outcrops of chert occur in a variety of different contexts, including nodules, discrete lenses, and layers in sedimentary deposits. Chert source localities can be unique, isolated outcrops, or part of a long stratigraphic bed providing many kilometers of exposed sediments. And in the Arctic, a variety of geomorphological processes, including solifluction, erosion, and water and glacial transport, can spread lithic raw materials far beyond their immediate source locality. The Precambrian crystalline rocks of the Canadian Shield contain a wide variety of cryptocrystalline lithic materials, including metamorphosed volcanics, sedimentary rocks, quartzite, and chert deposits as well as younger intrusive rocks (Bostock 1970), that are potentially suitable for making flaked stone tools. In the High Arctic archipelago, folded Mesozoic and Paleozoic strata contain igneous intrusions and sedimentary rocks (overlying the Precambrian basement complex) that contain outcrops of usable chert, slate, and quartzite deposits (Stockwell et al. 1970).

Stone tool assemblages are the cornerstones of cultural chronologies in the Eastern Arctic and the principal means by which cultural evolution and change have been discerned and interpreted. Stone tool assemblages have been analyzed from a functionalist perspective to determine prehistoric technologies and site function. Group identity and regional and interregional social relationships have been postulated on the basis of stylistic affinities in certain classes of stone tools. Throughout the Arctic, functional, technological, and stylistic studies of stone tools have served as the primary basis for constructing Paleoeskimo culture history (McGhee 1979; Maxwell 1985; Schledermann 1990). Even when bone and wood are recovered, stone tool typologies provide the *lingua franca* of Paleo-eskimo archaeology in the Eastern Arctic.

Analysis of stone tool assemblages nearly always includes discussion of the lithic raw material that prehistoric peoples used to fashion their implements. This,

in turn, has led to research directed at identifying these lithic sources (Bryan 1950; Clark and McFadyen-Clark 1993; Ericson and Purdy 1984; Findlow and Bolognese 1982; Luedtke 1976; Sieveking et al. 1972). Through study of lithic source localities and recognition of cultural lithic preferences for manufacturing projectile points and other chipped stone tools, archaeologists are provided with one of their best opportunities to look at prehistoric regional and interregional exchange and interaction systems, and the means to examine the social dynamics of trade and procurement patterns. Knowledge of the spread of lithic raw materials from their source localities is perhaps the best way that archaeologists can gain insight into the spatial dimensions of prehistoric cultures—the size, location, and durability of group territories and settlement patterns—as well as into group affinities and affiliations. Furthermore, differential access to lithic raw materials offers an opportunity to look at the emergence of hierarchical social structure through the use and control of exotic materials.

Through the identification and analysis of lithic raw material preference and use, it is possible to move beyond studies of cultural chronology, subsistence, and technology to get at notions of social systems and group identity. Social interaction is frequently inferred both by the stylistic affinities of stone tools and by the presence of exotic raw materials. In the Eastern Arctic, chert use is frequently culturally idiosyncratic and diagnostic (e.g., Maxwell 1973:48 in Baffin Island; Fitzhugh 1977a in Labrador) in that Paleoeskimo peoples predictably chose specific exotic chert sources even though appropriate materials were closer at hand. Indubitably, chert varieties were used to signal some form of social identity. The need of arctic foragers to maintain access to neighbors and resources is readily evidenced in the astonishing variety of exchange systems and systems of reciprocity that are featured in ethnographic observations. Furthermore, the need to facilitate access to information and distant social networks is essential for arctic

peoples living in sparsely populated landscapes. For prehistorians denied the evidence of ceremonial feasting, food sharing, and ritual paraphernalia, exotic lithic artifacts remain as tantalizing clues of such events. The challenge for archaeologists is in knowing how to interpret these phenomena.

Despite this potential for expanding our understanding of the prehistoric cultures of the Arctic, the sourcing of lithic raw materials has not figured significantly in Eastern Arctic research. A brief (not exhaustive) review of the literature reveals that while some researchers describe the local (apparently) dominant raw material in their chipped stone assemblages, for example, Wintenberg (1939:90, 1940:328) at Newfoundland Dorset sites; Maxwell (1973) for Baffin Island; Taylor (1968:15) for the Ungava coast and northeast coast of Hudson's Bay; Schledermann (1990) for Ellesmere Island; and Meldgaard (1952:222) for the Sarqaq assemblages from West Greenland, it is only recently that researchers have begun to describe and assess the source localities of exotic materials in their Paleoeskimo assemblages (Nagle 1986; Odess 1996). Other researchers ignored the raw material of their chipped stone assemblages entirely, for example, Leechman (1943) and O'Bryan (1953) in Hudson's Straits; Collins (1956a) on Southampton Island; Rowley (1940) at Abverdjar near Igloodik; Mary-Rousselière (1964) at Pelly Bay; and Knuth (1967) in northernmost Greenland. To be fair, this methodological lacuna is in part attributable to the episodic and wide geographical spread of Eastern Arctic archaeological research (to say nothing of the fiercely independent nature of arctic archaeologists), and the paucity of research projects prior to ca. 1970. Until recently, transportation costs and logistical constraints have inhibited wide geographical coverage in the Arctic of the sort that might facilitate a regional and interregional perspective on chert acquisition and consumption. A further hindrance to identifying sources of lithic raw materials is that the baseline geological mapping of much of the Eastern Arctic is yet in its infancy.

Notwithstanding the preceding historical qualifications, there is now available the cumulative testimony of over a half-century of archaeological investigations spread across the Eastern Arctic, as well as collaborative geological data and analytical procedures that could be mustered to address the questions of cultural affiliation and dynamics inherent in determining the sources and distributions of lithic raw materials.

Sourcing Lithic Raw Materials

Determining the source of lithic raw materials has figured significantly in a wide array of archaeological investigations (Luedtke 1992). Recognition of the potential research benefits of such analyses is predicated on the ability of matching artifacts with geological source samples. Frequently, however, lithic identifications are anecdotal, based on individual knowledge and experience. Such "eyeball analyses" (Luedtke 1993) are notoriously inaccurate given the similarity of some lithic materials (especially cherts!) and the tendency to underestimate source variability (Calogero 1992). The uniformity of chert chemical composition (being nearly entirely silicon dioxide) can also obscure analysis. Even within a single source, chert often has a wide color and texture variation that can make specific attribution difficult to determine.

With the following caveat in mind, chert identifications are frequently defined by visual macroscopic identification based on color, luster and translucency, macrofossil inclusions, and grain size (texture). While archaeologists regularly become familiar with local lithic types, there is an increasing likelihood of error in identifying similar-looking materials over a wide area. The likelihood of error is compounded in the Eastern Arctic where the low density of archaeologists and the large geographical distances between archaeological sites precludes a fine-grained site mosaic.

The uncertainty with strictly visual determinations of chert identification has led to the utilization of analytical methods based on chemical and petrographic

traits (Shotton 1970) and on trace element analysis (Luedtke 1978, 1987) for determining petrological and geochemical chert "signatures." Petrographic thin-sectioning and microscopy reveal the distinctive microcrystalline orientation of chert samples as well as diagnostic microfossils and carbonate composition (Prothero and Lavin 1990; Luedtke 1979, 1987). Additional techniques include neutron activation analysis and x-ray diffraction to provide chemical data on minor and trace elements that occur in different proportions in different chert deposits (Aspinall and Feather 1972; Luedtke 1979; Sieveking et al. 1972; Spielbauer 1984) and electron microprobe analysis of mineral inclusions to define mineral and element composition (Kempe and Templeman 1983; Malyk-Selivanova and Ashley 1995).

The potential for the recognition of regionally distinct chert sources is apparent from a few brief references in the Eastern Arctic literature. For example, Maxwell (1960:7) discusses the paleozoic quartzites and greywackes and low-grade chert that was available throughout northeastern Ellesmere, and the "excellent" (but not described) chert in the gravels of the adjacent Greenland coast. On Baffin Island, Maxwell (1973) mentions the local availability of small cobbles of some tan cherts. On Southampton Island, Collins (1956b:68) reports that a grey chert, available as nodules in the limestone formations of the southeastern shore, is representative of 99 percent of the stone tool inventory at the T-1 Early Dorset site. Unfortunately, none of these discussions quantify the nature or the amount of exotic materials or go beyond a casual description of the lithic types.

With more than seventy years of archaeological research, there now is some weight to the accumulated knowledge pertaining to prehistoric arctic occupations. Collections now housed in Canadian, English, and Danish museums provide a basis for making comparative observations on the utilization of lithic raw materials throughout the Eastern Arctic. A study of these archaeological assemblages could reveal the

range of local lithic preferences throughout the entire Paleoeskimo sequence and provide the basis of an archaeological database to compare with samples of chert and other siliceous stones from geological sources. This is an exciting direction for future research in the Eastern Arctic that has the potential to explore cultural processes on a broad geographical scale.

Lithic Procurement Strategies

An understanding of the lithic procurement strategies of prehistoric arctic peoples holds great promise for moving beyond the narrow confines of established regional culture history. Some indication of this potential can be realized from a brief inspection of the significance of lithic procurement studies in the Paleoindian literature (Ellis and Lothrop 1989).

Both Paleoindian and prehistoric arctic peoples can be characterized, at least in their initial pioneering stage, as highly mobile colonizers with low population densities and challenging environmental constraints. Long-distance trade in exotic materials serves both to meet the demand for nonlocal necessities and to operate as a social mechanism to avert regional resource vagaries in hunter-gatherer adaptations in marginal environments (Gould 1978:289, 1980; Hayden 1982; McBryde 1984). Because Paleoindian lithic choices frequently did not conform to least-effort acquisition strategies, researchers have looked beyond narrow technological and utilitarian explanations to explain the presence of exotic raw materials (Ellis 1989). In the Paleoindian literature the use of exotic lithic raw materials has been interpreted as a means by which widely dispersed populations were kept in contact with one another (Wilmsen and Roberts 1978:177-179), as a stylistic means to signal group identity (Ellis 1989:156), as a resource anchor about which dispersed groups would predictably aggregate (Gardner 1977: 260), and as a measure of social flexibility and mobility of settlement-subsistence strategies (Meltzer 1984, 1989).

While much of the Paleoindian literature pertaining to lithic raw material procurement is bogged down in

debates over scheduling decisions, the acquisition and transportation of lithic raw materials, even over great distances, do not seem likely to have inhibited prehistoric arctic peoples. Northern native peoples early on perfected the technological means to traverse large distances by developing watercraft and dog-team traction. Seemingly audacious travels by northern natives, facilitating the distribution of goods and social interaction throughout the Arctic, are a stable feature of northern prehistory and ethnography (Rowley 1985). Long-distance trade has been recognized as a recurring leitmotif in the Western Arctic (Burch 1988b; Nagle 1984; Stefansson 1914a).

The Paleoindian debate over the scheduling decisions pertaining to the acquisition of lithic raw materials includes perceived constraints imposed by snow and ice cover and frozen ground for would-be quarriers. While these difficulties may be true in some temperate localities with especially heavy snowfalls, they would tend to be offset in the Arctic where wind keeps much of the ground relatively free of snow cover and where both snow and ice greatly facilitate travel. Pep Wheeler (1900-1974), Labrador's preeminent pioneering geologist, was fond of noting "that the windswept uplands offered more rock exposure in winter than the unglaciated southeastern United States at any season" (Morse 1977).

Lithic Sources and Procurement Strategies in Labrador

That lithic raw material preferences could be correlated with distinct cultural and temporal aspects of Labrador prehistory was realized by William Fitzhugh during his dissertation research in Hamilton Inlet (Fitzhugh 1972b). Much subsequent research in Labrador has been devoted to sourcing and describing the varieties of lithic raw materials used by prehistoric peoples, including the study of a wide variety of locally available stones (quartz, slate, and nephrite) and steatite (Allen et al. 1978; Allen et al. 1984; Nagle 1982, 1984). With the accelerated pace of research in Labrador

during the 1970s, identifying lithic sources became a high priority. Cherts recovered from prehistoric sites in Labrador include the grey-banded Mugford cherts (Gramly 1978) and Ramah chert (Gramly 1978; Lazenby 1980) from the mountainous north coast. Chert sources from the Quebec-Labrador interior include Saunders chert, probably from the Seal Lake vicinity (McCaffrey et al. 1989), and the grey-green-tan cherts of the Sokoman-Ruth-Wishart and Fleming chert formations in the Labrador Trough region of north-central Quebec-Labrador (McCaffrey 1989a, 1989b).

Of all the wide variety of lithic raw materials in Labrador, none is so intimately associated with the prehistory of the region as is Ramah chert. Pioneering Maritime Archaic hunters and their families were the first to discover the Ramah chert quarries sometime around 7000 B.P., and its use became a prominent feature of the succeeding Maritime Archaic cultural sequence. Subsequent to the Maritime Archaic period, Ramah chert was an important feature of Groswater Dorset and Labrador Middle and Late Dorset cultures and was the nearly exclusive choice of the late prehistoric Indian cultures in Labrador. While archaeologists may sometimes be criticized for placing such a disproportionate interest in prehistoric lithic assemblages, it is not surprising given the visibility of chipped stone industries in the archaeological record. Nor is it surprising that a lithic raw material as beautiful and practical as Ramah chert would attract attention.

In the remainder of this chapter I explore the use and distribution of Ramah chert in order to assess its potential for elucidating prehistoric cultural dynamics in the "far Northeast" and as an example of the potential for similar studies throughout the Eastern Arctic.

On the Ramah Chert Trail

A lifelong interest in stone-tool manufacture and use led the antiquarian Sir Daniel Wilson to view collections and visit prehistoric quarrying sites in eastern North America and to correspond actively with colleagues at the Smithsonian and the Geological Survey of

Canada.² In a discussion of prehistoric lithic acquisition and distribution, Wilson provides the first reference to Ramah chert in the literature:

[This] suitable and specially prized material were sometimes sought on different sites, and disseminated from them by the primitive trader. Along eastern Labrador and in Newfoundland arrow-heads are mostly fashioned out of a peculiar light-grey translucent quartzite. Dr. Bell informs me that near Chimo, south of Ungava Bay, is a spot resorted to by the Indians from time immemorial for this favorite material; and arrows made of it are not uncommon even in Nova Scotia. (Wilson 1889:84-85)

Robert Bell, a geologist and naturalist for the Geological Survey of Canada's 1884-1885 expedition to Hudson's Bay, probably encountered Ramah chert while visiting the prominent site of Nunaingok near Port Burwell at the extreme northern tip of Labrador and at Nachvak Fjord (Bell 1884).

The peripatetic Warren King Moorehead is the next to comment on this distinctive lithic material. Long before his celebrated excavations of the Red Paint Cemeteries in Maine, he wrote:

A study of chipped implements . . . opens up a field of research of great possibilities. . . . For instance, chips of a certain stone, which appear to have come from Labrador, are said to be found occasionally in Maine or Massachusetts. If this statement is true, it leads us to question whether the Eskimo and the New England natives bartered, or whether there was a migration in earliest times from Labrador to New England, or vice versa. Or, whether the stone is found in New England as well as Labrador. (Moorehead 1910:249)

Fitzhugh (1972b:40) has suggested that Moorehead's Labrador derivation may possibly have originated from his knowledge of the Jewel Sornborger and Owen Bryant collections from northern Labrador and Alfred Kidder's collections from Newfoundland (all before 1910) at Harvard's Peabody Museum, probably brought to his attention by the museum's director Charles C. Willoughby. While Willoughby's excavations of several "Red Paint Indian" cemeteries did not produce any Ramah chert artifacts (Willoughby 1898), he

would have been familiar with specimens, including Ramah chert stemmed points, from other Maine cemeteries that were already in the Peabody collections (Smith 1948:34, 68). Furthermore, the use of red ocher in the burials inclined the New England antiquarians to look to Newfoundland, the ancestral home of the Beothuk, as a logical place of cultural origins (Willoughby 1898:52).

Recognizable artifacts of Ramah chert first figured in Moorehead's (1922:105) *A Report on the Archaeology of Maine* and, subsequently, in Willoughby's (1935:53) *Antiquities of the New England Indians*, both of which featured plates with half a dozen stemmed points from Red Paint Indian graves in Maine and, in Willoughby's book, a large biface from Rhode Island. Moorehead (1922:97) called it "Labrador stone" and wrote, "We took from the graves [at Lancaster's on the Kennebec] also a number of spear heads of translucent quartzite, that peculiar unidentified material which is common in Labrador but has never been found in a natural state, a ledge or boulder, in the State of Maine." Willoughby (1935:51) describes the stone as "a translucent quartz interspersed with nearly black blotches and shadings of gray, a material apparently foreign to New England and only occurring in these states so far as known in the form of finished blades. The source of this material is apparently in Labrador and possibly also in Newfoundland."

It was, I believe, William Duncan Strong who, as the anthropologist with the Rawson-MacMillan Subarctic Expedition of 1927-1928, finally provided, if not the actual quarry source location, then conclusive proof in the form of well-documented archaeological assemblages of the Labrador derivation for Ramah chert (Strong 1930). Strong collected from several coastal sites between Nain and Hopedale where he recovered bifacially worked stone tools and debitage of a distinctive raw material that he called "translucent chalcedony." Strong's "Old Stone Culture" was a mélange of artifacts from what we now know to be a number of separate Paleoeskimo and prehistoric Indian cultures.

One location Strong collected from was a site at Sharp Hill in Big Bay, about halfway between Hopedale and Nain. Here an outcrop of fine-veined quartz had been quarried in antiquity leaving the surface littered with quarry debris as well as debitage from an occupational episode. Strong believed that both the quartz and “chalcedony” had been quarried from outcrops on Sharp Hill. The geology of the central Labrador coast precluded the likelihood that an outlying bed of Ramah chert could occur here, but Strong’s suggestion was enough of a nagging concern that Fitzhugh invested considerable time in revisiting the site locality and eventually put to rest the specter of a separate southern outcrop of Ramah chert (Fitzhugh 1972b:42, 1974).

In 1934, Junius Bird excavated several Labrador Eskimo winter houses during his honeymoon in Labrador (Bird 1945). Beneath the house floor of one structure Bird uncovered points and flakes of the distinctive stone. Bird had previously made several voyages along the northern Labrador coast, first in 1927 as a member of the Putnam expedition to Baffin Island (Putnam 1928) and later with Captain Robert Bartlett, during which he had seen flakes of Ramah chert at sites in Eclipse Harbor and Newfoundland. The American Museum of Natural History, where Bird worked, also had a few Ramah chert bifaces from the Maine Red Paint Indian sites.

By the century’s midpoint, it was generally conceded that Moorehead’s “Labrador stone,” the “translucent chalcedony” of Strong and Bird, was indeed derived from Labrador, although the actual provenance was yet unknown. It was Elmer Harp (1964b:255–256) who finally resolved the mystery of the source for this raw material, much of which he had seen in the course of fieldwork in southern Labrador and Newfoundland. Harp’s research was framed within the context of Eskimo origins and the nature of Indian/Eskimo relationships, and he commented on the “widespread and persistent occurrence throughout the marginal north-east of translucent grey quartzite as a major raw material for chipped artifacts” (Harp 1964b:255). He noted the prevalence of this material in some of his

collections from the Strait of Belle Isle and in the Red Paint burial sites from Maine.

Following World War II, the mineral potential of the Labrador peninsula attracted considerable attention. Through conversations with British Newfoundland Exploration, Ltd. (BRINEX) geologists, Harp learned of the presence of a broad band of translucent grey quartzite centered in the mountainous fjorded region of northern Labrador at Ramah Bay. Comparison of geological samples with archaeological specimens determined that, at last, the fabled source of what has come to be called Ramah chert was located. According to Harp’s BRINEX informant, an Inuit to whom the material was shown, it looked like *tunnuyakh* (caribou back fat).

The Ramah chert trail next gets picked up by Fitzhugh who became familiar with the material during his dissertation research in Hamilton Inlet in 1968 and 69. Fitzhugh (1972b:40–44, 239–244) provides the first detailed description of the raw material, including a physical and chemical analysis and a description of the source localities. A history of the geological research and mapping of the Ramah series is in Morgan (1975). A detailed inspection of the quarry site by archaeologists was made in 1976 and during the Torngat Project research in 1977 and 1978. Descriptions of the Ramah chert quarry are in Gramly (1978) and Lazenby (1980). Lazenby (1984) also summarizes the geology of the Ramah chert source locality in the context of her study of Maritime Archaic chert use in Labrador.

Ramah Chert in the Far Northeast

Given the prominent role that Ramah chert plays in Labrador prehistory, this chapter frames the prehistoric distribution of Ramah chert in the context of the cultural sequence in Labrador, essentially from the perspective of the residential cultural anchor, the Labrador starting point, from which chert distribution must have proceeded. Divided into four principal periods, these are: (1) the Maritime Archaic, ca. 7000 to 3500 B.P.; (2) the Paleoeskimo sequence, including Groswater Dorset, ca. 4100–2100 B.P., and Early-

Middle-Late Dorset in Labrador, ca. 2500–800 B.P., but excluding Pre-Dorset components; (3) the Late Prehistoric Period Indian cultures, ca. 1800–400 B.P.; and (4) Norse activity in the New World, ca. 1000 B.P.

Maritime Archaic

By 7000 B.P., intrepid Maritime Archaic hunters, probing the margins of the known world, discovered the spectacular Ramah chert outcrops. Early Maritime Archaic sites in southern and central Labrador have chipped stone assemblages that are characterized by quartz, red quartzite, and slate industries, essentially local lithic materials. With the discovery of the Ramah chert sources, Ramah chert became the preferred chipped-stone material, a preference that increased with time until it became the nearly exclusive choice in Late Maritime Archaic Rattlers Bight complex sites (4000–3700 B.P.; Lazenby 1984). The use of Ramah chert by Maritime Archaic groups in Labrador peaked at the same time as regional expressions of an elaborate mortuary tradition known from Labrador,

Newfoundland, and the maritime Northeast. Excavations of a Maritime Archaic village and associated cemetery at Rattlers Bight in Hamilton Inlet (Fitzhugh 1976c), ca. 4100–3500 B.P., revealed stone-lined burial pits filled with stone and copper artifacts, sheets of mica, and walrus ivory, all covered and stained with brilliant red ochre. Ramah chert bifaces, stemmed points, quarry blanks, and flakes were included as burial furniture in several of the Rattlers Bight graves. Although nearly identical to specimens recovered from the nearby occupation site, the Ramah chert flaked-stone assemblage from the burials was frequently larger and in

pristine condition in comparison with resharpened and reused specimens from the village area.

Approximately 600 kilometers further north, at Nulliak, lies the largest Maritime Archaic site on the north coast (Fitzhugh 1981). The site at Nulliak dates to ca. 4300 B.P. A scant sixty kilometers from Ramah Bay, it must have facilitated access to the chert quarries. At Nulliak there are a number of long-houses and at least two large stone-capped burial mounds. Ramah chert artifacts, especially large numbers of stemmed points, were recovered from both domestic and mortuary contexts (fig. 14.1).

Middle and Late Maritime Archaic sites have been discovered along the central and southern Labrador coast and in the adjacent near-interior. These sites attest to the pervasive reliance on Ramah chert by Maritime Archaic Indians to meet their chipped-stone needs. Ramah chert artifacts with close stylistic affinities to the stemmed points and large bifaces from Rattlers

Bight and Nulliak have been recovered from sites on the north shore of the Strait of Belle Isle, at Forteau Bay, and at the mouth of the Pinware River (Harp 1964b). These sites also contain Ramah chert debitage that testifies to the transport of Ramah chert as a lithic raw material,

in addition to the artifacts that appear to have been brought in finished form from the north. As we will see, artifacts of Ramah chert extend far beyond the Straits region but, significantly, only as carefully finished stemmed points and semilunar bifaces. The Straits appear to mark the southern boundary of groups that had direct access to Ramah chert, either through procurement expeditions to the north or through exchange with closely allied groups. While the situation is not yet clear on Newfoundland, where few Maritime Archaic sites have been excavated, it is apparent that south and west of the Strait of Belle Isle



14.1/ *Ramah chert artifacts from Nulliak*

the transportation of Ramah chert is limited to carefully crafted objects of ceremonial significance.

On Newfoundland there is yet to be an excavation of a Maritime Archaic habitation site on par with those conducted in Labrador. Two cemetery excavations, at Port au Choix (Tuck 1976a) and Twillingate (MacLeod 1967), however, provide dramatic testimony to the continuity of a shared mortuary tradition linking sites in Newfoundland with those in Labrador, the Maritimes, and Maine. Three radiocarbon dates from the Twillingate burials average 3500 B.P., contemporaneous with the occupation at Rattlers Bight. Several Ramah chert artifacts, including a stemmed point and the portion of a semilunar biface, were recovered from the Twillingate burials. Ramah chert debitage was recovered from limited testing at an adjacent habitation site.

While no Ramah chert artifacts were recovered during the cemetery excavations at Port au Choix, a remarkable cache of Ramah chert bifaces was previously found on a beach terrace just below the Maritime Archaic cemetery (Harp 1964a:141-144). The cache, discovered in 1946 by Walter Billard while preparing his garden, consisted of seventy-three chipped stone implements, including sixty-four Ramah chert artifacts (seventeen broad leaf-shaped bifaces, six semilunar forms, thirty-seven unifaces, and four biface fragments). It is impossible to tell at this late date whether this material was originally interred as part of a mortuary feature or whether it is indicative of some other ritual or ceremony. Three other caches of Ramah chert bifaces have been recovered, two in southern Labrador and one on the Quebec North Shore; they are discussed in further detail below.

Moving up into the Gulf of the St. Lawrence, the fourth millennium B.P. use of Ramah chert appears to drop off precipitously. Whether this perception is a result of the paucity of research in the area or a historical reality only the test of time will tell. To date there is a

single Ramah chert stemmed point reported from near Trois Rivières (Wright 1982:200) and another one from "New York."³ Wright (1995:194) reports that some Ramah chert "specimens and flakes" have been found as far west as Cornwall, Ontario, but no provenance is provided.

While the use of Ramah chert in a Late Archaic context seems to diminish as one heads deeper into the interior toward the Great Lakes, the situation is dramatically different along the maritime coast south of Newfoundland into New England. There is a lacuna between Newfoundland and the coast of New Brunswick and Maine where Ramah chert artifacts have yet to be reported from a Late Archaic context. Interest-

ingly, this gap coincides with a gap between the Late Archaic Maritime cemeteries of Newfoundland and Labrador (Tuck 1971) and the obviously allied Moorehead Mortuary complex cemeteries (Sanger 1973), Moorehead's Red Paint Indian cemeteries, in New Brunswick and Maine. Within this ceremonial mortuary context Ramah chert stem-med points and semilunar bifaces are a recognized but rare feature (fig. 14.2). Of the nine distinctive traits that Moorehead (1930: 47) applies to his description of the Red Paint Indian culture in Maine, he includes "spear heads of clear chalcodony known as the Labrador stone." These artifacts are clearly manufactured in Labra-



14.2/ Ramah chert stemmed point found eroding out from a probable burial feature at Indian Island, Old Town, Maine⁴

dor and traded south in a context that maintains their significant symbolic value and importance. When documentation exists, Late Archaic Ramah chert artifacts appear to be derived exclusively from mortuary/ceremonial contexts.

Ramah chert artifacts remain extremely rare throughout the region: in the Maritimes only three stemmed points have been located in antiquarian collections (Patricia Allen, personal communication 1987). Stemmed points and large bifaces of Ramah chert, however, are a dramatic component of Late Archaic mortuary traditions in Maine, having been commented on

by numerous researchers who have worked in the area (Bourque 1971; Moorehead 1922; Robinson 2001; Smith 1948; Snow 1980; Willoughby 1935).

While most of the Maine cemetery sites were excavated prior to the advent of radiocarbon dating, recent excavations at two sites demonstrate that they are contemporaneous with Rattlers Bight and Nulliak occupations (Belcher et al. 1994:21; Snow 1975:50). Other than these mortuary finds there is only a sparse scattering of Ramah chert artifacts that have been identified in New England: (1) a semilunar Ramah chert biface acquired by the Smithsonian in 1868 from a site on Grand Lake Stream, St. Croix River, Maine⁵ (fig. 14.3a); (2) the mid-section of a large Ramah chert semilunar biface recovered from the central Connecticut River Valley in the town of Hadley, Massachusetts, about seventy years ago (fig. 14.3b); and (3) at least four large bifaces from Rhode Island, apparently the southernmost appearance of Ramah chert during the Late Archaic period. The provenance of two of these specimens is only “Rhode Island” (Willoughby 1935:51); of the other two, one is from North Smithfield⁶ (fig. 14.4) and one from Wakefield (fig. 14.5).

Paleoeskimo Archaeology

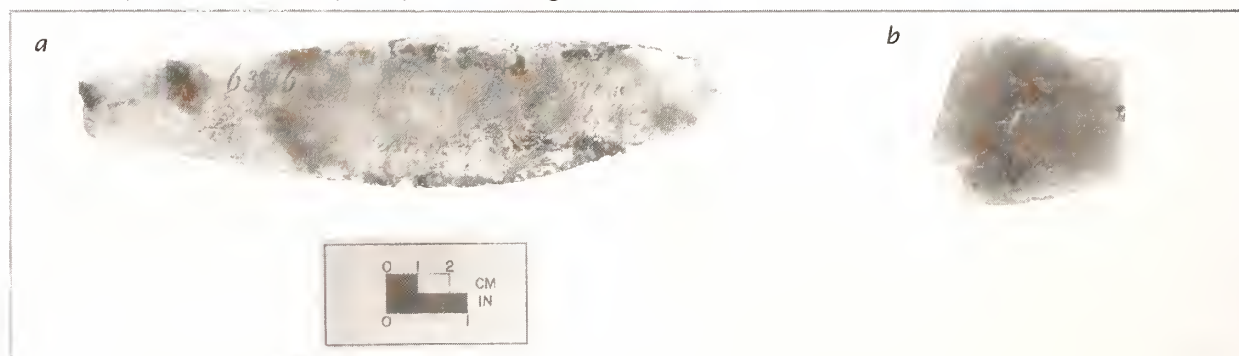
In Labrador, the use of Ramah chert by Paleoeskimo populations increases dramatically through time. Ramah chert occurs sparingly in some Pre-Dorset assemblages (4100-3300 B.P.); the pioneering Paleoeskimo population in Labrador preferred the finer-grained Cape Mugford cherts. However, transitional Groswater

Dorset groups (2800-2200 B.P.) proved extremely eclectic in their consumption of lithic raw materials, with varying amounts of local (Ramah) and exotic (Cow Head cherts from Newfoundland’s Northern Peninsula, first mentioned by Wintemberg [1939:88]) cherts in their assemblages. Groswater Dorset components at Nunaingok, at the extreme northern tip of Labrador, contain tools and debitage derived from Newfoundland sources. Digging at Nunaingok in 1935, Douglas Leechman found Paleoeskimo components full of “quartzite”—Ramah chert. Leechman (1943:365) wrote, “the source of the quartzite is not known to the modern Eskimos, who use fragments from the old village site when they have need of it.”

Further south, at Postville (Loring and Cox 1986) on the central Labrador coast, 500 kilometers from the Ramah quarries and nearly a thousand kilometers from Newfoundland, the Groswater Dorset stone tool assemblage is composed of approximately 70 percent Newfoundland cherts and 25 percent Ramah. At the southern terminus of Groswater Dorset culture, at the Strait of Belle Isle on the Quebec North Shore (Pintal 1994:151; Plumet et al. 1994) and in Newfoundland (Auger 1986:113; Carignan 1975:47; Renouf 1994:174), Newfoundland cherts dominate the assemblages, although a very small proportion of tools and debitage made of Ramah chert are always present.

Clearly, during the Groswater Dorset period in Newfoundland and Labrador the acquisition and generous consumption of chert from distant sources attests to the presence of fairly formal, elaborate, and

14.3/ Large Ramah chert bifaces from New England





14.4/ *Ramah chert biface from North Smithfield, R.I.*

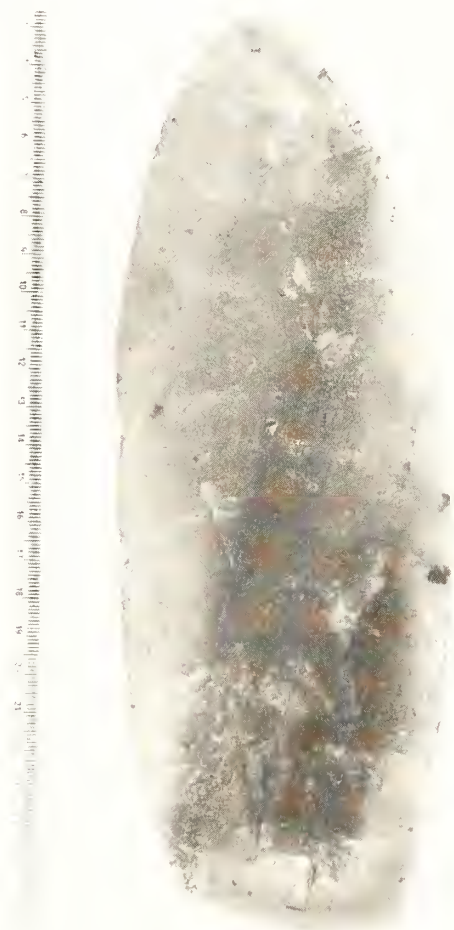
sophisticated exchange and interaction networks. These networks, it has been hypothesized, served as a means to circumvent the constraints imposed by a linear coastal-maritime-settlement-subsistence strategy through reciprocity and kinship relations (Loring and Cox 1986:78).

With the advent of the Late Paleoeskimo tradition (ca. 2500–800 B.P.), the “classic” Early-Middle-Late Dorset of Labrador (Cox 1978; Tuck and Fitzhugh 1986), Ramah chert becomes the nearly exclusive lithic choice for flaked-stone tools and continues so until the Thule appropriation of the coast signals the end of Dorset culture (Nagle 1986). The lithic technology of Thule peoples consisted primarily of a ground-slate industry. Occasionally, in northern Labrador, we find small water-washed and/or ground and polished chunks of Ramah chert at Neoeskimo house sites, the purpose and significance of which must await further analysis. Nagle (1986) has written on the nearly exclusive use of Ramah chert by Dorset Paleoeskimos in Labrador. He tests Renfrew’s (1977) distance-decay model, quantifying the nature of Ramah chert consumption and use in relation to increased distances from the quarry location.

Newfoundland Dorset, contemporaneous with Middle Dorset in Labrador, is most characterized by its particular regional stamp (Harp 1964a). Harp (1964a:91) describes the occurrence of Dorset artifacts made of “translucent grey quartzite” and others of “flint or chert” but source identifications are not hazarded. With the recognition of the northern Labrador source for Ramah chert, it is apparent that there must have been some

interaction between Dorset populations in Newfoundland and Labrador; small quantities of Newfoundland cherts occur at Middle Dorset sites in the Nain area and further north (Jordan 1986:142).

North from Labrador it is rare to find site reports detailed enough to include the analysis of debitage and quantify the appearance of exotic lithic raw materials. In western Ungava Bay, Plumet’s Tuvaaluk Program proves the exception to the rule. He and his colleagues (Desrosiers 1986; Labrèche 1986a; Plumet 1986b, 1994) note that Ramah chert, as well as other exotic materials, occur in trace amounts at the Paleoeskimo sites at Diana Bay. On Baffin Island, as part of the Meta Incognita Project in outer Frobisher Bay, Smithsonian researchers located several Early and Late Dorset sites some with a few flakes or a few tools of Ramah chert (Odess 1996, 1998).



14.5/ *Ramah chert biface from Wakefield, R.I.*

Further west at Nuvuk, a Dorset site near Cape Wolstenholme, Nouveau Quebec, Leechman (1943: 366) hints at the presence of Ramah chert and of material likely to be derived from Southampton Island. At Southampton Island, some 1,200 kilometers from the Ramah chert quarries, Cox (1978:113) reports that Henry Collins's collections from T-1 contain "a few Ramah chert tools." This latter observation is especially interesting given Cox's claim of close similarities between the Early Dorset component at T-1 and Early Dorset sites in Labrador.

Late Prehistoric Period Indian Archaeology

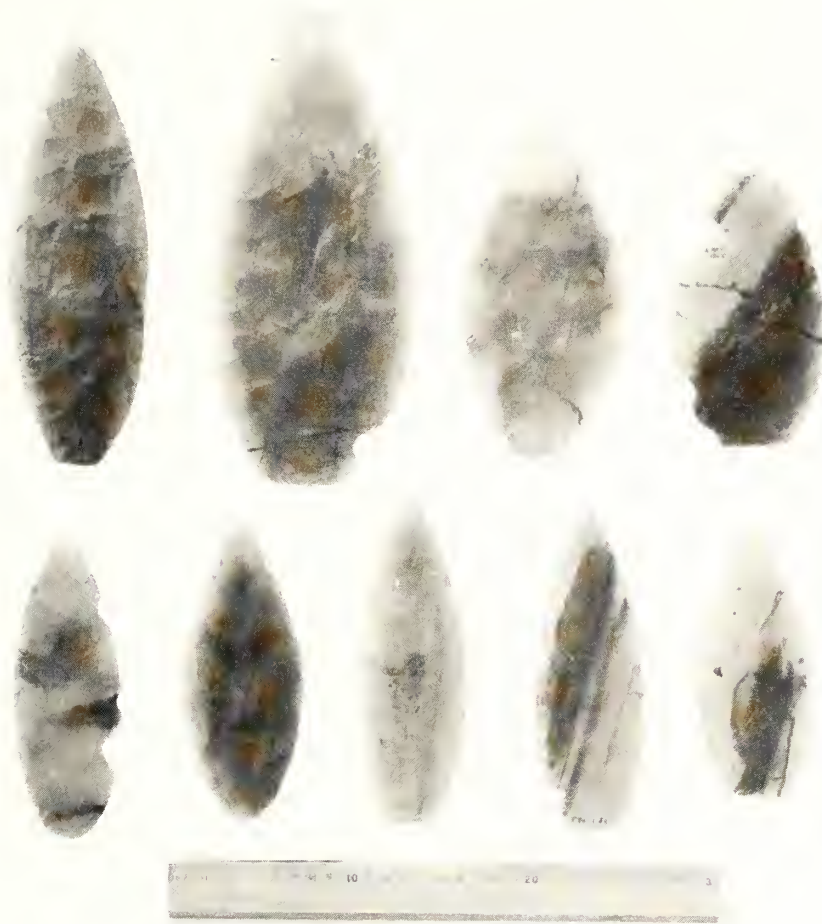
Labrador's Indian prehistory is broken into three episodes. The initial Maritime Archaic period, ca. 7000–3500 B.P., is followed by a series of Intermediate Indian occupations, ca. 3500–2800 B.P. (Nagle 1978), and finally by the Late Prehistoric period with its Daniel Rattle and Pt. Revenge complexes, ca. 2000–400 B.P. (Fitzhugh 1978b; Loring 1988a, 1992). Coeval with Middle and Late Dorset Paleoeskimo occupations in northern Labrador, Late Prehistoric period Indian groups (the ancestors of the Innu) frequented the central Labrador coast and adjacent interior. While I have not encountered any contemporary traditions among the Innu that pertain to the use or knowledge of Ramah chert, it was the nearly exclusive lithic preference of their ancestors. This passion for Ramah chert necessitated a journey far to the north of the tree line to an alien world inhabited by strangers—a journey fraught with dangers.

Late Prehistoric period Indian stone tool assemblages are characterized by the conspicuous consumption of Ramah chert. Along the central Labrador coast, Daniel Rattle (ca. 1800–1000 B.P.) and Pt. Revenge (ca. 900–300 B.P.) complex sites are frequently found littered with large amounts of Ramah chert debitage (Loring 1992). The early Daniel Rattle components have a mixed bifacial and unifacial chipped-stone tool assemblage. The bifacial industry consists primarily of straight-based lanceolate forms and side-

notched projectiles. The unifacial industry consists of a wide variety of quite large side and end scrapers and flake knives. This unifacial industry might have been an excellent technological strategy to maximize the potential use-life of stone tools, a valuable strategy for highly mobile, dispersed hunters and gatherers who lived hundreds of kilometers south of the Ramah chert quarries. Such economic assumptions, however, are negated by the expansive squandering of large amounts of Ramah chert in the form of debitage at these sites. Clearly, late prehistoric Indian groups in Labrador had no problems in getting large quantities of Ramah chert.

There is little evidence that Late Prehistoric period Indian groups lived north of Nain, although a thin trickle of diagnostic projectile points extends all the way to the southernmost extension of the Ramah quarries at Saglek. Rather, the north coast of Labrador was the homeland of Middle and Late Dorset peoples with whom Daniel Rattle and Pt. Revenge people must have been in contact. Dorset culture disappears around A.D. 1300 with the sudden appearance of Neoeskimo Thule invaders. Whatever social relations may have existed between Late Prehistoric period Indian and Paleoeskimo peoples were irrevocably severed.

With Thule cultural expansion along the entire Labrador coast, eventually extending all the way to Newfoundland, the Indian socioeconomic landscape was radically altered. Ramah chert retained its prominence in the lithic assemblage at late Pt. Revenge sites, but the flagrant consumption, characterized by the large volume of debitage in the earlier Daniel Rattle components, was superseded by apparent stinginess and intensive reworking and reuse of available materials. The latest radiocarbon-dated Pt. Revenge site is Aly's Head in Hamilton Inlet (Fitzhugh 1978b:159–160; Loring 1992:354–358). Charcoal from a hearth produced a date of 325 ± 80 (SI-1276) equivalent to A.D.1625. By this time the Thule and European appropriation of the coast appears to have been a factor in the withdrawal of Indians from a maritime setting and



14.6/ Casts of the Spingle cache bifaces held by the Archaeology Unit, Memorial University, St. John's.

coincidentally from their economic and ceremonial-symbolic identity with Ramah chert.

Late Prehistoric period Indian sites extend the full length of the central and southern coast of Labrador as far as the Quebec North Shore. At Blanc Sablon, a number of prominent Late Prehistoric period Indian sites with Ramah chert artifacts and debitage have been recorded (e.g., the Kidder collection at Harvard's Peabody Museum [Fitzhugh 1972b:plate 87a, e-k]; the Lawrence Jackson collection [Loring 1985:132-133]; materials collected by Harp [1964b]; and recent excavations conducted by Jean-Yves Pinal [1989, 1998, personal communication 1992, 1998]).

One of the more unusual characteristics of Ramah chert distribution during this time period is the appearance of three caches of Ramah chert bifaces, two from southern Labrador and one from the Quebec North

Shore. In the fall of 1995, a spectacular cache of "about 90" large Ramah chert bifaces were found by a hunting party "between Port Hope Simpson and William's Harbour" along the southern Labrador coast (Pomeroy 1995). Details remain sketchy but newspaper photographs reveal an astonishing array of large Ramah chert unifacial tools and bifaces. Superficially, these artifacts bear a striking resemblance to tools from the Late Prehistoric period Daniel Rattle components at sites near Davis Inlet and Postville and to material recovered from the Spingle cache. A second extraordinary cache of Ramah chert artifacts from southern Labrador included at least nine remarkable bifaces,

several biface fragments, and a number of flakes; it was discovered by Gordon Spingle in 1990 while gardening in front of his home in L'Anse-au-Clair, Labrador, on the Strait of Belle Isle (fig. 14.6). The Spingle bifaces are large (average length is 193 mm) and broad (maximum width is 113 mm; average width is 77 mm) with pronounced convex sides. The bifaces do not neatly slip into previously described categories. The lack of any associated materials makes their attribution difficult, but I believe they date to the Late Prehistoric period on the basis of their similarity to bifaces in the Stubbett cache, as discussed below.

The Stubbett cache of Ramah chert bifaces was found by Huey Stubbett in the village of Kegashka, on the Quebec North Shore approximately 350 kilometers west of the Strait of Belle Isle (Chism 1982; Loring 1992:446-449). The Stubbett cache consists

of twenty-nine large Ramah chert bifaces, an unworked tabular piece of Ramah chert, a biface of dark gray quartzite, and a polished stone rod of uncertain function (fig. 14.7). Several Stubbett cache bifaces are identical to lanceolate forms recovered from Daniel Rattle and Pt. Revenge sites in Labrador and to a specimen recovered from a cache of bifaces found in Saybrook, Connecticut (see below). Others include broad-bladed bifaces with convex sides, which in turn are similar to the bifaces in the Spingle cache. Maritime Archaic people also produced large Ramah chert bifaces, including lanceolate forms (Harp 1964:243), but the absence of rectangular, semilunate and bipointed forms diagnostic of the Maritime Archaic period (Fitzhugh 1975:127, 1978a:78), and the recovery of both the small lanceolate bifaces and the large, narrow dagger-like forms from well-documented

Daniel Rattle components, support the attribution of the Stubbett cache to the Late Prehistoric period. The similarity of the broad bifaces with convex, almost round, blade outlines links the Stubbett and Spingle caches although, barring supportive further data, this attribution must remain tentative.

These three caches and the high percentage of Ramah chert utilized at Late Prehistoric period sites on the Quebec North Shore are not predicted by gradual fall-off models of down-the-line exchange (Renfrew 1977). Such dense accumulations of Ramah chert 1,600 kilometers from its source are not concentrated by hand-to-hand, trickle-down exchange but rather by highly motivated, direct procurement activities by individuals or small groups. In order to overcome the difficulties in bringing Ramah chert such a distance, there must have been a substantial social/ideological

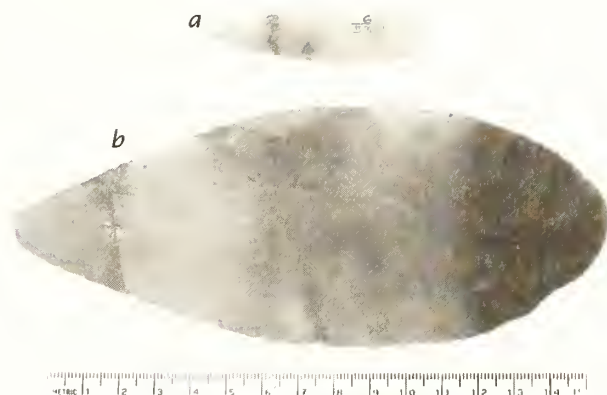


14.7/ Stubbett cache as photographed by William Fitzhugh in Kegashka, summer 2001

investment in the chert that would make it preferable to less distant raw materials. Further to the south and west, the value or significance of Ramah chert does not figure so centrally in the socioeconomic aspects of group identity.

Across the Straits in Newfoundland, Late Prehistoric period Indian cultures (a.k.a the Recent Indian period in Newfoundland) ancestral to the Beothuk appear to be closely allied with their Labrador neighbors. And while Ramah chert does not figure significantly in most of the Newfoundland assemblages, the marked stylistic convergence of the Labrador and Newfoundland stone tools attest to some interregional exchange and interaction. Ramah chert side-notched projectile points have been recovered at the Beaches site in Bonavista Bay (Carignan 1975:105, plate 26) and other Beaches assemblages, dating roughly between A.D. 800–1200 (Loring 1992:456–459). At the Bank site (DdAk-5)—an important Recent Indian site also in Bonavista Bay—a linear hearth feature was excavated that contained an impressive amount of Ramah chert tools and debitage, leading its excavator to suggest that the acquisition and consumption of exotic materials, like Ramah chert, may have figured significantly in ritual feasts and ceremonies (Schwarz 1992).

Moving west from the Strait of Belle Isle up the St. Lawrence estuary, Ramah chert is repeatedly found in small amounts at some Late Prehistoric period sites along the lower Quebec North Shore. Most often it occurs as isolated finds. Large unifacial Ramah chert scrapers, similar to specimens from the Daniel Rattle complex sites in Labrador, have been recovered near the Saguenay, at the Sainte-Marguerite River (Levesque 1962:23) and at Trois-Rivières (Marois and Ribes 1975:60, 95–96). Kidder and Tuck (1972) found Ramah chert debitage associated with small corner-notched projectile points from a mixed multicomponent site on Anticosti Island, and a small Levanna-like triangular arrowhead of Ramah chert was recovered from beside the Richelieu River north of Lake Champlain (Wright 1979:32–33).



14.8/ Bifaces from Vermont at the National Museum of the American Indian

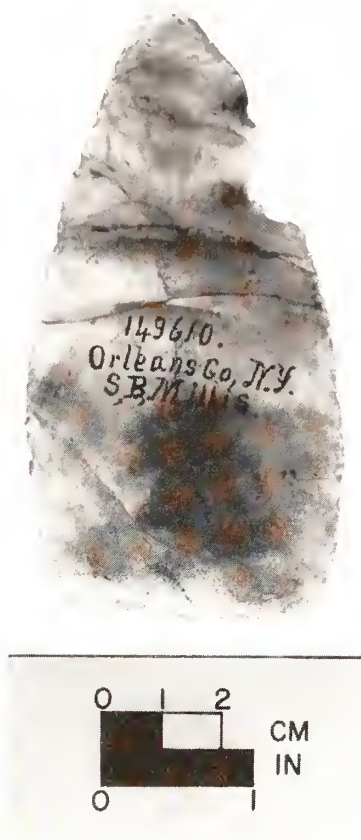
From the Champlain Valley there are a pair of small bifaces of uncertain cultural/chronological attribution that are likely associated with this Late Prehistoric period distribution of Ramah chert. The first is a small Ramah chert flake point in the collection of William Benton of Vergennes, Vermont, which was found at the mouth of Otter Creek on Lake Champlain. While there is little doubt about the lithic material, its cultural attribution is less obvious as the flake point has stylistic affinities to flake points from the Maritime Archaic habitation site at Rattlers Bight in Hamilton Inlet (Fitzhugh 1972b, plate 79 a-p) and Windy Tickle near Hopedale (Strong 1930:plate 4 n-t). The second is a cylindrical-shaped biface or drill, a form that has no counterparts further north; it was found in the collections of the National Museum of the American Indian but contains no additional information besides its Vermont provenance (fig. 14.8a).

Perhaps the most interesting piece of Ramah chert to come out of Vermont is a large ovate biface recovered in 1895 from Barker Farm in Leicester, Addison County (fig. 14.8b). While the Late Prehistoric period attribution is uncertain, this biface could well be a Late Archaic specimen, its close affinity to bifaces in the Spingle cache from L'Anse-au-Clair, Labrador, makes a Late Prehistoric period attribution possible.

West of Vermont's Lake Champlain, the Late Prehistoric period trade in Ramah chert appears to drop off perceptibly, perhaps attesting to the emergence of less permeable social boundaries between the more

mobile Algonquian groups and incipient Iroquoian villages. However, an intensive survey of old museum collections might likely change this perspective and demonstrate further mechanisms of social interaction than heretofore perceived. For instance, while working through the collections of miscellaneous artifacts in the holdings of the Canadian Museum of Civilization, Jean-Luc Pilon (1999) reports finding three small lanceolate Ramah chert bifaces from two different sites on the lower Gatineau River that had been collected prior to 1936. Pilon likens the Gatineau River finds to Meadowood cache blades but to this author they bear a very strong resemblance to the small straight-based bifaces found at Daniel's Rattle complex sites in Labrador (Loring 1985: fig. 7; 1992). And in the collections of the Smithsonian Institution, there is a large square-based Ramah chert biface that was found in Orleans County, New York (near Lake Ontario) in 1893 (fig. 14.9).

With the diminution of the Ramah chert trail to the west, we return to the Maritimes to pick up the trail anew. It seems unlikely that individuals from Labrador would ever have traveled much beyond the Strait of Belle Isle. In the absence of direct contact and interaction there is, nevertheless, a diffusion of some materials and ideas, as Late Prehistoric period Indian sites in the Maritimes share a number of strong stylistic features with sites in Newfoundland and Labrador (Loring 1988b). While it has to be recognized as fundamentally different from the direct long-distance exchange and interaction that occurred among Indian groups further north, the late prehistoric cultures of the Maritimes, including Keenlyside's Maritime Woodland and the Ceramic period sites in Maine, contain provocative data on the distribution of Ramah chert during the Late Prehistoric period in the Northeast.



14.9/ Ramah chert biface
from New York

Ramah chert is very scarce in collections from the Maritimes according to David Sanger (personal communication 1987) and Stephen Davis (personal communication 1987). However, occasional flakes and locally manufactured tools of Ramah chert have been recovered: (1) Moira McCaffrey (personal communication 1994) reports locating several flakes of Ramah chert in the course of survey work on the Iles-de-la-Madeleine; (2) on Prince Edward Island, Ramah chert tools and debitage are a consistent feature of Late Prehistoric period Indian sites dating ca. 1050–850 B.P. (Keenlyside 1982, 1984; Keenlyside and Keenlyside 1976:30); and (3) on the New Brunswick mainland, traces of Ramah chert are also present at

Late Prehistoric period sites.⁷

In the course of documenting archaeological collections on Prince Edward Island, David Keenlyside was shown a remarkable, small triangular projectile point that appears to be made of Ramah chert (personal communication 1995)⁸ found along the Tracadie River. Typologically, this specimen seems similar to some Late Dorset endblades from northern Labrador coast. If it is indeed a Paleoeskimo artifact, then it joins an intriguing set of Paleoeskimo objects transposed from their northern point of origin (realizing a prediction made by the naturalist Alfred S. Packard [1885:473] more than a 100 years ago). Steven Cox has identified a Dorset ground and polished burin-like tool in the collections from the Goddard site, a Late Prehistoric period habitation site on the central Maine coast, and also a pair of Dorset bone harpoon heads from Smith and Wintenberg's (Smith and Wintenberg 1929:plate XX 1–2) excavation of the Merigomish shell heap in

Nova Scotia (Bourque and Cox 1981:24–25). As the Paleoeskimo artifacts from Maine and Nova Scotia are associated with Late Prehistoric period Indian shell middens they may have functioned as gift items or curiosities that signaled, as did Ramah chert, something exotic and desirable. They remain tangible evidence of the elaborate social mechanisms that linked Indian bands throughout the Far Northeast.

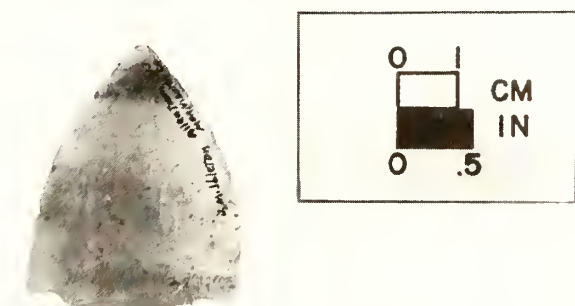
There are several Late Prehistoric period sites in New Brunswick along the Tracadie River that, according to David Keenlyside (personal communication 2000), have produced Ramah chert debitage and artifacts. One of these sites, the Savoie site (CiDf-11), produced a Ramah chert assemblage that included several hundred biface thinning flakes and a half-dozen or so scrapers and small bifacial knives (Keenlyside and Keenlyside 1976). The late prehistoric use of Ramah chert at the Savoie site is dated to 1025 ± 120 (SI-713).

The sparse trail of Ramah chert leading to New England becomes a bit more conspicuous when we reach the state of Maine. A number of Ramah chert artifacts have been recovered from a variety of late prehistoric Ceramic period sites along the central Maine coast: at the Jones Cove shell heap (Smith 1929:8) and the Watson site (Cox and Kopec 1988), both in Frenchman's Bay; a shell heap in Casco Bay (Arthur Spiess, personal communication 1989); and the Goddard site on Blue Hill Bay (Bourque and Cox 1981). These coastal sites all appear to be coeval with occupations ca. 1000–700 B.P. They have typical Late Ceramic period assemblages of small side-notched

projectile points made out of both local and exotic lithic raw materials, including Ramah chert specimens (Kopec 1987). At both the Goddard and Watson sites a high percentage (30 percent at Goddard) of the lithic raw materials are derived from non-local lithic sources, including cherts from western New York, Vermont, and the Bay of Fundy, and jasper from Pennsylvania (Cox and Kopec 1988:42). More than 150 flakes of Ramah chert, including large preform reduction flakes and small bifacial resharpening flakes, and at least thirty Ramah chert artifacts (made into local styles of side-notched projectile points and end scrapers) were found at the Goddard site (Bourque and Cox 1981:15; Steven Cox, personal communication 1989).

Ramah chert has also been recovered from several Late Prehistoric period interior sites in Maine: Steven Cox reported two flakes of Ramah chert in a large collection from Mattawamkeag on the upper Penobscot and several tools (a distal biface fragment, two end scrapers, and a couple of flakes) in a collection from Grand Lake Stream, a tributary of the St. Croix (Steven Cox, personal communication 1989); and Arthur Spiess and Robson Bonnicksen report finding a piece of Ramah chert near Munsungun Lake in 1980 (Arthur Spiess, personal communication 1989).

Eventually, the southerly trend of the Ramah chert trail peters out in southern New England and the mid-Atlantic states but not before some surprising manifestations. A lanceolate biface of Ramah chert was recovered as part of a cache found near the mouth of the Connecticut River at Saybrook, Connecticut, around 1942 (Loring 1992:484). The cache consists of twelve large, mottled-yellow-brown jasper bifaces, a parallel-sided, straight-based Ramah chert biface, and several rolled copper beads. Frequently, the attribution of caches composed of unfinished bifaces is problematic. In this case, however, the stylistic similarities of the Ramah chert lanceolate biface from the Saybrook cache with bifaces recovered from Daniel Rattle complex sites in Labrador unequivocally link the two in time. The distal portion of a broad-bladed Ramah



14.10/ Monmouth County, NJ, biface



14.11 / *The southernmost Ramah biface known to date was found in Maryland.*

chert biface (fig. 14.10) with convex edges and what appear to be small side notches was recovered in Monmouth County, New Jersey, and was formerly in the Dorothy Middleton collection (Gary Fogelman, personal communication, November 2000). The convex blade outline has no clear Labrador antecedents and may be a form produced locally by a mid-Atlantic Middle Woodland tool manufacturer. And finally, the presently recognized most southerly occurrence of Ramah chert is a large, impressive biface (fig. 14.11) found in Riverton, Maryland, and formerly in the Judge William Yates collection of Cambridge, Maryland (Fogelman 1997; personal communication, November 2000). Without closer inspection, it is difficult to ascertain whether this specimen has a Maritime Archaic or Late Prehistoric period association.

Ramah Chert and Vikings

Persistent, but inconclusive, references to the presence of a pair of Newfoundland-Labrador corner-notched projectile points recovered from Norse sites in West-

ern Greenland are tantalizing suggestions of another form of culture contact (Berglund 1981; McGhee 1984a; Rowlett 1982). One specimen, possibly made of Ramah chert, was found in 1930 at Sandnes in Vesterbygdén (Roussell 1936:106); the second, made of quartz, was a stray find recovered from rocks on the shore below the Norse ruins at Brattahlid, the very site from which Thorfinn Karlsefni left on a Vinland expedition in 1003 (Meldgaard 1961). A recent report of Ramah-like quartzite from East Greenland potentially complicates this situation (Gulløv and Rosing 1993). But, as the now famous recovery of a Norwegian penny—minted between A.D. 1065 and 1080—from the Goddard site in Maine (Bourque and Cox 1981) attests, small forgotten objects can, by their context, eloquently attest to complex historical processes and events.

Questionable Associations of Ramah Chert

Finally, there are a number of references to the occurrence of Ramah chert that have surfaced in the literature that I believe need to be discredited. Anecdotal references sometimes have a way of entrenching themselves, no matter that the evidence is strictly hearsay. In his initial discussion of Ramah chert in the Hamilton Inlet monograph, Fitzhugh (1972b:40) makes reference to the appearance of Ramah chert artifacts recovered from as far away as Maryland and Florida (repeated by Lazenby [1980:632] and Wright [1995:194]). The Maryland find, which Fitzhugh heard about from James Tuck, appears to be the Judge William Yates specimen previously referred to. The Florida specimen was reported by the Labrador geologist Everett Wheeler. Fitzhugh himself never saw these artifacts, and I have been unable to affirm the Florida attribution.

In Vermont, several bifaces from the Boucher site, an Early Woodland cemetery near Swanton, have erroneously been identified as being made of Ramah chert (Haviland and Power 1994:98). Instead, they are almost certainly Mistassini quartzite from Lac Albanel in central Quebec (McCaffrey personal communication; Heckenberger et al. 1990).

It is interesting to speculate why the Ramah chert trail does not appear to penetrate into the Great Lakes region of the midcontinent. The St. Lawrence seems every bit a natural highway as do routes along the coast. Yet a casual examination of museum collections (Boston, New York, Washington) has yet to ferret out Ramah chert specimens, beyond the one western New York biface. The westernmost distribution of Ramah chert is attributable to a Maritime Archaic bipointed biface found near Peterborough, Ontario (Moira McCaffrey, personal communication).⁹ Other than the previous reference to specimens seen by J. V. Wright (1995:194), and the specimens reported by Pilon (1999), there are no reports of Ramah chert from Ontario (Michael Spence, personal communication 1988) or the Great Lakes region (K.C. Dawson and Ronald Mason, personal communications 1987). Mason no longer stands behind his statement that Ramah chert artifacts have been recovered at Shield Archaic sites in the Great Lakes (Mason 1981:138).

Although Haviland and Power (1994:63) believe a “close relationship” exists between the Vergennes Archaic of the Champlain Basin and the Maritime Archaic of the Far Northeast, I am less convinced. If we look at the distribution of exotic materials recovered from Archaic sites in Vermont, the lack of any significant numbers of artifacts made of Ramah chert or other products from the Maritimes, coupled with the surprising quantities of copper tools (cold hammered from Lake Superior nuggets) in antiquarian collections, suggests Vermont Archaic social relations more likely took a westward orientation. Late Archaic interregional cultural dynamics remain among the most intriguing problems in North American archaeology. Pioneering studies on the distribution of raw materials far from their sources have significantly structured perceptions of eastern United States prehistory (e.g., Seeman 1979; Griffin 1965). Quantifying the nature and dynamics of long-distance exchange (of both raw materials and artifacts) has provided archaeologists with one of their

best means to approach questions of precapitalist economies, territoriality, and the emergence of political autonomy and authority.

Discussion

In this chapter, I have hoped to demonstrate the potential that the study of the acquisition and distribution of lithic raw materials holds for enlightening perceptions on the social dynamics of prehistoric cultures. (It is also an oblique testimony to the value inherent in old museum collections.) The absence of discussions of lithic raw material variability and use, of descriptions of lithic sources, and of analyses of raw material percentages and composition of assemblages is, with some exceptions, the norm in the archaeological literature of the Eastern Arctic. Such studies and analyses, however, would seem to hold the promise of revealing the intensity (or lack thereof) of interregional contact and exchange among dispersed arctic populations as has been suggested by this review of the use and distribution of Ramah chert. After nearly a century of speculation, much of the mystery about Ramah chert has been resolved. It remains for the next generation of scholars to articulate the mystery for further revelation of prehistoric adaptations in the Eastern Arctic and the Far Northeast. The distribution of Ramah chert challenges assumptions about the boundedness of arctic and subarctic peoples, invites new theories for modeling group interaction and interregional contact, trade and communication, and the boundaries of social groups. Some indication of these directions can be inferred from the following concluding notes.

Ramah Chert Distribution during the Late Maritime Archaic Period: ca. 4500–3500 B.P.

The consumption of Ramah chert in Maritime Archaic sites in Labrador is an entirely different proposition from its appearance and use at Moorehead period burials in Maine and the Maritimes. In Labrador, Ramah use transcends domestic and ceremonial life; it is

the raw material used in a wide variety of cutting and scraping tools recovered from midden and house excavations and, as chunks of raw material, flakes, stemmed points, and a variety of large biface styles, it is found in ocher-stained burial pits. South of Labrador, Ramah chert loses its mundane connotations entirely. There is no evidence that Ramah chert was being transported as a raw material; rather, classic Labrador forms—stemmed points, semilunar bifaces, and lanceolate bifaces—went south to be “consumed” in an exclusive mortuary context.

The actual number of Ramah chert points and bifaces in the Maine burials is, after all, small and could be the result of a single procurement/acquisition event. In this respect, the Ramah chert situation is somewhat analogous to the appearance of Yellowstone obsidian in Ohio Hopewell assemblages where the spectacular nature of the raw material and the drama inherent in its appearance so far from its source overshadows the fact that the actual amount of raw material is slight (Griffin 1965:146). So, while the temptation is to see the transportation of Ramah chert to New England as part of a formal long-distance exchange network, it seems equally likely that the Ramah chert in the Maine cemeteries could stem from a unique event or from several casual encounters. The exclusive appearance of Ramah chert in Maine mortuary features suggests that special individuals were being selected for extraordinary treatment. These were individuals who were able to parlay their knowledge, reputation, skills, or prestige to gain access to exclusive materials. In thinking about the evolution of tribal identities, Bender (1985:23) links social behavior and material culture with “leadership geared to specific subsistence activities,” and with mediation and decision-making pertinent to “alliance, marriage and exchange.” The specialized nature of some Maritime Archaic activities, specifically the dangerous activities associated with long-distance voyages, deep-sea fishery, and hunting large marine mammals, would necessitate special leadership roles and organizational authority. Such individuals might

acquire specialized knowledge of distant peoples and resources.

In a discussion of Maritime Archaic symbolic traditions, Fitzhugh (1985c) has suggested that many Indian groups in the Northeast share a common outlook that links spiritual identity with individual practices and beliefs. The lack of rigid ceremonial practices (suggested by the variability in regional Late Archaic burial conventions) parallels the relatively informal social hierarchies that epitomize the loosely knit band structure of subarctic Indian groups. In a similar sentiment, Bourque (1994) has questioned adaptational models of interregional exchange “systems,” suggesting alternatively that trade might result from unique historical events initiated by adventuresome individuals seeking personal power and prestige. Such a scenario might better explain the cluster of Ramah chert stemmed points in Maine and the Ramah chert bifaces in Rhode Island.

For hunters, with their intimate knowledge of environment and local resources and their profound belief in the spiritual component of killing animals, it is not too far-fetched to link the symbolic ideological significance of stone projectile points with social power, recognition, and prestige. This is evident in later Early and Middle Woodland societies in the Northeast where large, exotic bifaces figure prominently in mortuary ceremonialism. I have suggested that the prevalence of large bifaces in Early-Middle Woodland ceremonial features indicates their seminal role in ritual behaviors, forming a symbolic medium that was recognizable over a large area and among dispersed groups (Loring 1989).

That these Ramah chert stemmed points were not passed along as heirlooms but in every case with good provenance were “consumed” as mortuary offerings suggests that they represented objects of significance to individuals, not necessarily evidence of an established or sustained formal association between widely separated contemporaneous groups.

Ramah Chert Distribution during the Late Prehistoric Period: ca. 1800–400 B.P.

The Late Prehistoric period distribution of Ramah chert among northeastern Indian groups is quite different from that of the preceding Maritime Archaic period. The patterns of Ramah chert occurrence in New England and the Maritimes never suggest actual chert procurement expeditions launched from the south. As detailed above, the fourth millennium B.P. distribution of Ramah chert throughout the Far Northeast is limited to specimens manufactured by Maritime Archaic Indian groups in central and northern Labrador and subsequently dispersed to the south as finished objects. With the Late Prehistoric period distribution of Ramah chert in the Northeast, however, there is evidence of both the transport of Labrador-manufactured bifaces (the biface from western New York, the specimen from the Saybrook cache, the Yates biface) as well as the transport of Ramah chert as a raw material. This latter interpretation is supported both by the recovery of Ramah chert flakes, evidence of tool manufacture, and by chipped-stone artifacts made into local (non-Labrador) styles. The transport of raw material appears to signal a different mechanism of distribution than that of the preceding Maritime Archaic period. The distribution of Ramah chert from northern Labrador is one means we have to question the rigidity, permeability, and continuity of prehistoric group boundaries. With a variety of quality, flakable lithic raw material available from local sources, the choice to acquire exotic raw material is a social and ideological decision, not just an economic one.

As with the preceding Maritime Archaic cultures, Late Prehistoric period Indian groups in Labrador had a nearly exclusive reliance on Ramah chert for the manufacture of their chipped-stone assemblage. Ramah chert was critical to the success and the definition of social and economic systems in Labrador. South of Labrador and the Quebec North Shore and in Newfoundland and the adjacent Maritime Provinces, local lithic raw materials are the preferred choice for tool manufacture, so

that Ramah chert is not as likely to have such socio-economic significance.

It seems plausible that exotic materials like Ramah chert would come attached to knowledge and information that had social connotations. In Labrador and along the Strait of Belle Isle, early Late Prehistoric period Indian populations would have had contact with coeval Middle and Late Dorset groups and competed for access to certain coastal resources. The large quantities of Ramah chert in the collections from the Strait of Belle Isle and the adjacent Quebec North Shore signal strong, direct channels of trade and communication with Indian groups in Labrador. This interpretation is further strengthened by the presence of caches of Ramah chert bifaces, which could be construed to indicate direct procurement or acquisition of Ramah from the source in northern Labrador. Caches suggest control over a valued resource. Such an interpretation argues for the existence of a strong Indian identity with allegiance to nonlocal groups as a hedge against subsistence shortfalls and ethnic competition. In Maine and the Maritimes, Ramah chert would no longer provide the critical means of social integration and regional interdependence that it clearly did along the Strait of Belle Isle and the Quebec North Shore (where the percentages of Ramah chert in site assemblages are very high), but the information that accompanied the raw material would serve to define relationships between groups and prevent rigid social and territorial boundaries from forming. Lacking preservation of exotic materials—food, plant materials, medicine, fur—chert may be, as Barbara Luedkte (1987:45) has called it, “the tip of the ‘trade iceberg.’”

A social system that facilitated the distribution of exotic raw materials remained in place throughout the Late Prehistoric period in the Far Northeast. The large, square-based Ramah chert bifaces recovered from the caches near Blanc Sablon, at Kegashka, and in Saybrook, Connecticut, are early diagnostic forms at Daniel Rattle complex sites and date to ca.1800–1400 B.P., while the small, notched Ramah chert projectile points from

sites in the Maritimes and New England postdate 1000 B.P. At the Goddard site, Ramah chert was distributed throughout the Ceramic Period occupation so that its presence is not indicative of just a single procurement episode (Bourque and Cox 1981).

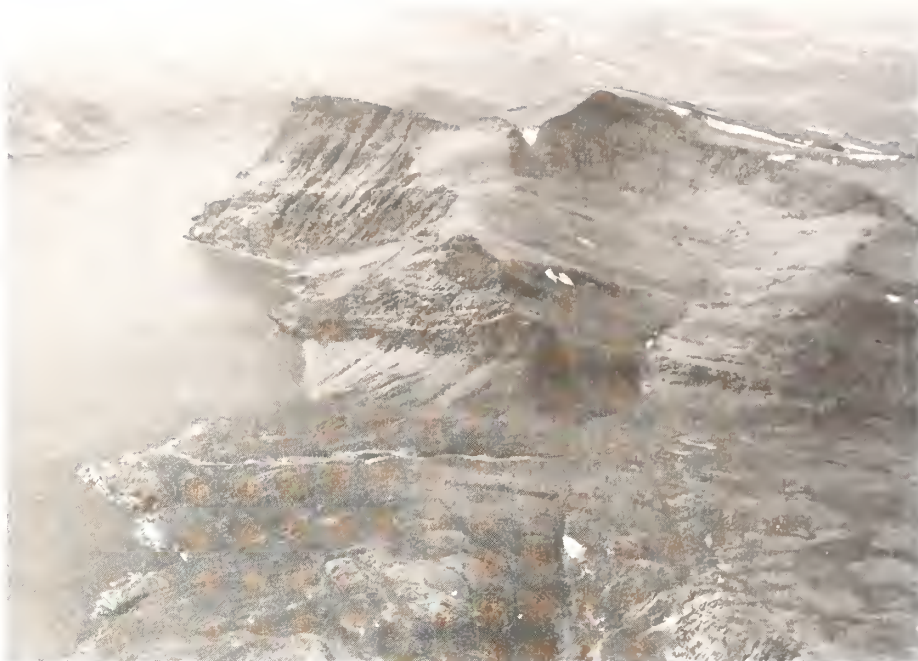
To the best of my knowledge, the southern Ramah chert trail ends with the bifaces recovered in the Say-brook, Connecticut cache and

with the Yates biface from Maryland. These artifacts have traveled nearly 3,500 kilometers from their source in Labrador's Torn-gat Mountains. They remain a tantalizing testament to the power of material objects to evoke wonder and amazement, even in such disparate social contexts as a feature in a Late Woodland ceremony and as objects of twenty-first-century academic speculation.¹⁰

Ramah Bay and Ramah Chert

Before leaving Labrador it seems appropriate to consider the less tangible dimension of Ramah chert acquisition and use. Given the pervasive spiritual dimension in the lifeways of northern hunters and the prominence of Ramah chert use, at least by prehistoric Indians and Paleoeskimos in Labrador, it is inconceivable that the material, and the place from where it was derived, would not have been laden with spiritual significance. Ritual and ceremony would have been an integral feature of procurement activities. In considering the spiritual landscape attendant on Ramah chert procurement, I offer the following observation.

Some of the most accessible and highest quality chert at Ramah is to be obtained along the walls of a



14.12/ *The quarry cirque at Ramah Bay, Labrador*

prominent glacial cirque carved into the mountain massif on the north side of Ramah Bay (fig. 14.12). The chert-bearing deposits are reached by following a stream that drains the cirque. The final approach to the quarry bowl passes through a dramatic band of iron-rich rocks that have stained the streambed and surrounding rocks a brilliant blood red. Here, the narrow stream valley is at its most constricted point with sheer cliffs rising on both sides. The symbolic pairing of the red-ocher-stained rocks with the source for the material with which the most sacred practice—the killing of animals—was intimately associated must have figured significantly in the telling of the story.

Acknowledgments

I suspect that Elmer Harp has long forgotten our first meeting when, in 1971, as an undergraduate from Goddard College in Plainfield, Vermont, I wandered down to the Dartmouth College Museum in Hanover to look through their boxes of arrowheads from northern New England as part of a B.A. thesis on Vermont archaeology. I hadn't met too many archaeologists then, so Elmer set the standard for politeness in the profession; at least he wasn't impatient at my

then decidedly antiquarian proclivities. My archaeological elders were Warren King Moorehead, C. C. Willoughby, Maurice Robbins, William Ritchie, and a cadre of odd Vermonters (Leslie Truax, William Ross, John Bailey, and Godfrey Olsen). Armed with a copy of Ritchie's *A Typology of New York State Projectile Points*, I was then attempting to unravel the prehistory of northern New England, arguably a prehistoric landscape every bit as remote and disjointed as that of the Eastern Arctic.

A very conspicuous thanks is due Moira McCaffrey, who not only has the coveted distinction of ferreting out the earliest reference to Ramah chert in the literature but was also responsible for bringing information on the Stubbart cache to my attention. I would like to acknowledge Noel Broadbent for translating the Berglund article for me and William Fitzhugh, Moira McCaffrey, and Daniel Odess for their comments on earlier drafts.

At least for some of us, Ramah Bay remains a haunted place, impossible to write about and not remember Anne Abraham, who disappeared there during the Smithsonian's initial reconnaissance in 1976. No one has dwelled in Ramah Bay since the Moravians abandoned their short-lived mission community (1871–1907). The Moravian grave markers in “God's Acre” have fallen so that only a few stones from the mission's foundation and the row of Inuit sod-house ruins remain. Time has a way of playing tricks in northern Labrador. No doubt, on their leaving, the Moravians were aware of the Old Testament passage, if not the irony, from 1 Kings 15:21–22: “and they took away the stones of Ramah.”

Notes

1. The quotation in the chapter title comes from Old Testament 1 Kings 15:21–22.

2. I am indebted to Moira McCaffrey for sleuthing out this reference.

3. In the Smithsonian Institution, NMAI #24/9538; no additional provenance data is available.

4. Found by Maurice Crandall ca. 1943. Three additional similar stemmed points and a distal portion of a large biface, all of Ramah chert, from this site are in the NMAI collections (20/2352).

5. In the Smithsonian Institution, NMNH #A-6376: G. A. Boardman collection, Milltown, Maine.

6. This item is from the J. H. Clark collection purchased in 1875. Clark acquired archaeological material from throughout southern New England. The biface is 101 mm in length and has a broad tip and a broad expanding blade; this is 49 mm wide at its shoulders where it forms an obtuse angle that becomes the stem with straight sides and base; it has heavily ground lower lateral and basal edges.

7. Large unifacial Ramah chert scrapers have been recovered at the Old Mission Point site in the northern part of the province and at the Howe site on the Northwest Miramichi River (P. Allen, personal communication 1987).

8. At the Annual Meeting of the Canadian Archaeological Association in Ottawa, May 2002, I had the opportunity to examine this artifact in the company of David Keenlyside, Rob Ferguson, David Denton, and Moira McCaffrey. We all agreed that the specimen certainly appears to be made of Ramah chert. The artifact was a surface find from the eroding Jones site on PEI found by Rollie and Jeanette Jones. It is clearly associated with a group of small, asymmetrical triangular projectile points with deep concave bases that have been recovered from the Jones site, as well as at other sites in New Brunswick and on the Magdelaine Islands, which are attributable to a Late Paleoindian tradition dating to circa 9000–10,000 B.P. (See also Tuck, 1984).

9. In the Royal Ontario Museum #22896, recovered from Concession 6, Carden Township, Victoria County, Ontario. The biface is missing one end and the surviving section is 23 cm long and 6.5 cm wide.

10. Least one suppose that Ramah chert artifacts remain exclusively in the purview of researchers and archaeologists—and Labrador's Innu and Inuit descendants of those who left the tools and debitage behind—it is worth noting that a Ramah chert biface figures significantly in William Sarabande's (1998) “First Americans,” a novel of the post-Pleistocene maritime Northeast.

History of the Archaeological Research in Arctic Quebec

PATRICK PLUMET

When William Fitzhugh suggested to me that I could grapple with the history of the archaeology of Arctic Quebec (now Nunavik), in which Elmer Harp had participated, I had to confess that, although having been deeply involved in active research in Quebec for more than twenty-five years, I had never found the time to step back from it to start such an exercise. Therefore, the offer was a welcome opportunity. I soon discovered that trying to understand the evolution of archaeology in northern Quebec could be as complicated and baffling, as subjective and sensitive, as any archaeological interpretation. In other words, it was fascinating. I quickly realized that I could not avoid dealing with the psychological, sociological, and political context of this subject. The following sketchy review needs more archival research and personal interviews, but it is, at least, a beginning. I have chosen to organize the discussion in three time periods: the international pioneering period; the Quebec period; and the Inuit period.

International Pioneering Period

The historically recorded geographic exploration of the Quebec-Labrador coasts began at least as early as Henry Hudson's 1619 voyage and Thomas Button's 1612 explorations. Nevertheless, little archaeological information was registered before the middle of the twentieth century. From 1920 to 1940, the European arctic expeditions, which established the first scientific framework of Eastern Arctic prehistory, closely approached, but never seemed to have entered, the



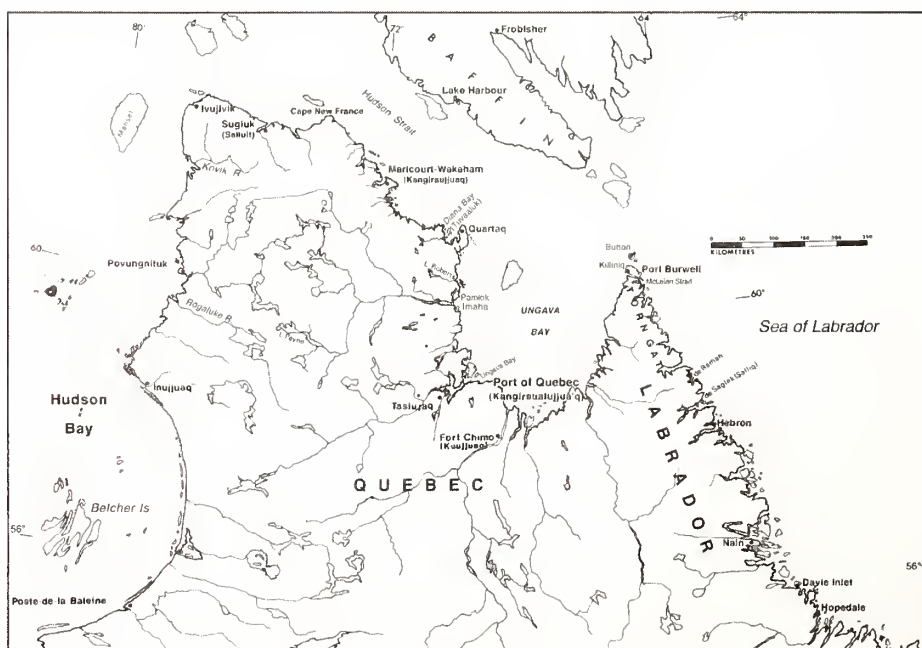
territory of northern Quebec. Although Therkel Mathiasen (1927a, b) and Diamond Jenness (1925) had referred to archaeological collections from the Quebec-Labrador Peninsula and from the Belcher Islands, and Lucien Turner's (1894) ethnological studies had laid the cultural groundwork for interpreting the recent Innu and Inuit past, this region was largely bypassed by explorers who were primarily interested in the Northwest Passage and by anthropologists who were largely concerned with whether the Eskimo culture originated in the central or western Arctic. It is important to note, however, that the Dorset culture was first identified by the Canadian anthropologist Diamond Jenness in 1925 from artifacts collected at Cape Dorset and Coats Island, which are close to northern Quebec. Unlike Labrador, where Duncan Strong (1930) and Junius Bird (1945) excavated prehistoric structures, ethnologists and geologists working in Ungava made few observations of archaeological interest before 1940. Some aspects of the history of research in eastern Ungava and in a northern Labrador have already been discussed in the context of the Tuvaaluk Project (Plumet and Gangloff 1990; see also Tuvaaluk's websites: www.unites.uqam.ca/tuvaaluk and unites.uqam.ca/nunavik).

Following the discovery of the Dorset culture, its origin and distribution became the hottest problem in Eastern Arctic prehistory. In 1935 and 1936, archaeologist Douglas Leechman (1943), working on the northeastern fringe of New Quebec at Nunaingok on McLellan Strait and on its northwestern margin in the Nuvuk Islands (fig. 15.1), discovered and tested Dorset sites. In 1939, based on artifacts collected in the Belcher Islands off the eastern shore of Hudson Bay, George I. Quimby (1940) described the Manitounik culture as a mixture of Dorset and Thule cultures. In 1944 and 1946 while conducting geodetical surveys, Thomas H. Manning (1948, 1951) noted Thule semisubterranean dwellings on several islands along the central and northeastern coast of Hudson Bay and on the mainland near the mouth of the Kovik River. He also passed along information he had learned from a trader about a group of Dorset houses on Sugluk (now Salluit) Island at the southwestern entrance of Hudson Strait.

The first archaeological information from the interior of the Ungava Peninsula came from Jean Michea, a French ethnologist who, like his fellow countryman Edgar Aubert de la Rüe, was a geologist, and from the French Canadian geographer Pierre Gadbois, who was a member of one of Jacques Rousseau's expeditions. Rousseau, a prominent ethnobotanist from Quebec, carried out extensive pioneering work throughout the entire Quebec-Labrador Peninsula from Lakes Mistassini and Alabanel north to the Korok and Allurilik Rivers. He also crossed the Tornat Mountains nearly to

Saglek on the Labrador coast. His fields of expertise included all aspects of the natural sciences as well as ethnology.

In 1948, Rousseau's expedition left Povungnituk on eastern Hudson Bay by canoe, traveling up the Kogaluk River to Payne Lake and then down Payne River to Ungava Bay (Rousseau 1948, 1949; Malaurie and Rousseau 1964). At the eastern end of Payne Lake the party discovered prehistoric sites that they estimated were of "extreme importance" (Michea 1950:55). Michea rapidly tested one site containing thirty houses and recognized three phases of occupation, one of which seemed to be Dorset. Clearly aware of the importance of his discovery, he noted that this was "the first time Dorset features have been known to occur so far from the sea" (Michea 1950:57). While Michea tested this site, Rousseau explored others. From Payne Bay, they visited Inuit camps along the coast between Cape Hopes Advance and Salluit. In his report, Michea mentions the existence of many archaeological ruins at Cape Hopes Advance, Diana Bay, Wakeham Bay, and Sugluk (Salluit), noting that most of them "had been excavated by the natives themselves for their own use so that their importance



15.1/ Map of Arctic Quebec, now called Nunavik.

for archaeology has been much reduced" (Michea 1950:58), but he does not give any site locations. At about the same period, in 1954, Swiss ethnologist Claude Desgoffe collected artifacts from semisubterranean dwellings that he thought conformed to Quimby's Manitounik culture (Desgoffe 1955a, 1955b). Shortly thereafter, Desgoffe drowned in a boating accident in the Belchers.

The year 1957 marked the beginning of archaeologically oriented research in Arctic Quebec, as well as a turn in Quebec history and mentality. Rousseau was appointed Director of the National Museum of Canada in Ottawa in September 1956 (fig. 15.2) and shortly thereafter instigated William E. Taylor's work in Ungava.

In 1957, Taylor began systematic archaeological explorations at Payne Lake in northern Ungava. On his way into the interior, Taylor stopped at Payne Bay, made a brief survey of the estuary, and spent one evening on Pamiok Island (Taylor 1958). According to what he told me much later, Rousseau had asked Taylor to investigate a different Payne Lake site than what Michea had visited, which he thought was very important. In the course of his thirty-seven days at Payne Lake, during which he tested six sites, Taylor failed to find Rousseau's site, and on September 6 flew out of the interior, and its cloud of

blackflies, to the more breezy Salluit area on southern Hudson Strait. There, until the beginning of October, he conducted preliminary work at Thule and Paleo-eskimo sites. During the next two years, he prospected and excavated Pre-Dorset and Dorset sites around the northwestern tip of the peninsula, at Salluit Island, Ivujivik, Mansel Island, and other locations in north-

eastern Hudson Bay (Taylor 1959). His reports and syntheses (Taylor 1964a, 1964b) are the first comprehensive treatment of Nouveau-Québec prehistory. Taylor's results confirmed and elaborated upon the suggestions that Elmer Harp had made earlier based on his work on the Dorset culture in Newfoundland: that Arctic Quebec was to be included in the Dorset and Pre-Dorset culture area; that data from the Arnapiik and Tyara sites demonstrated Dorset origins in Canadian Pre-Dorset (the subject of Taylor's 1968 dissertation); and that a stonewalled longhouse at the Pamiok site, which Taylor named Imaha ("maybe" in Inuktitut), might be Dorset. Taylor also excavated a burial containing a skeleton associated with Dorset artifacts and, for this

reason thought he had found the first Dorset human remains (Laughlin and Taylor 1960)! I discuss this burial further below. In any case, in 1964, Taylor wrote that "perhaps the more salient feature of Quebec archaeology is a monumental ignorance of it based on a ludicrous dearth of fieldwork."

At the end of the 1950s, a serious conflict exploded at the new Human History Branch of the National Museum of Canada between Rousseau, the first French Canadian to be appointed director, and some of his colleagues. The troubles had

been set off by the discovery in 1951, at a politically sensitive location, of the site of the Long-Sault battle by a young archaeologist named Thomas Lee. These conflicts made newspaper headlines in Ottawa and Montreal.¹

I believe that the eviction of Jacques Rousseau from the Museum has not been without consequence for



15.2/ Jacques Rousseau, Director of the Canadian Museum of Civilization

the development of archaeology in Quebec. After three years of teaching at the Sorbonne in Paris at Claude Lévi-Strauss's invitation, Rousseau returned from exile and entered the newly created Centre d'Études Nordiques (CEN) at Laval University. This center, whose founder and first director was Louis-Edmond Hamelin, was to take a leading role in scientific research in Arctic Quebec, including ethnology and archaeology.

The end of the fifties and beginning of the sixties were characterized by a new, and at first quiet, affirmation of the French Canadian identity: "la Révolution tranquille." For a community eager to act like a nation, archaeology can be one of the most innocent ways to reappropriate national identity and territory. Consequently, it was not accidental that French Canadian archaeology emerged among amateur groups at the very end of the fifties. In promoting scientific research in Arctic Quebec, Quebecois hoped to establish their right to what remained of the northern part of the province, which they called New Quebec and where Indians and Inuit had, for a long time, been ruled by the federal government. In 1961, with Georges-Émile Lapalme as the first Minister of Cultural Affairs of Quebec, an Archaeological Service was organized. Historical monuments were protected by a law inspired by the French "Loi Poincaré."

I came to Canada on December 2, 1962, after five months in Iceland and Greenland. I intended to stay only six to eight months in order to complete my research on Norse expeditions to the New World and I certainly did not expect to be attending the Elders Conference thirty years later. I became involved in Quebec archaeology immediately. Although I had been interested in archaeological research for a long time, especially in the Near and Middle East, in Canada I found an opportunity to work in remote areas where I could combine travel, science, and my interest in barren lands, whether hot or cold.

After I finished my Ph.D. at the University of Paris in 1966, Louis-Edmond Hamelin asked me to assist Thomas Lee on his next expedition in Arctic Quebec (fig.

15.3). In Quebec, I was probably one of the few French-speaking students trained in archaeology who also was familiar with Norse problems. Since 1964, Rousseau had asked the CEN to sponsor Lee's work because of his problems with the Human History Branch of the National Museum of Man. Lee had taught North American archaeology at Laval University and was subsidized by Hamelin to conduct archaeology at Payne Lake. Rousseau and his son Jérôme joined the team for a few weeks to help relocate the sites that had been discovered in 1948, which Taylor supposedly had missed in 1957 (this point was mentioned as important by Rousseau and by Lee; I add "supposedly" because Taylor did not agree with this point), and to orient Lee to their study. Once in the field, Lee found several new sites, including one that he claimed was a Viking settlement with a church. If the Norse had settled inland, it would be logical to expect to find some of their vestiges along the coast. With this possibility in mind and armed with information received from the Inuit, in 1966 Lee and I surveyed the west-central coast of Ungava Bay. We found nothing except the Pamiok site (figs. 15.4, 5), where Lee located three longhouses (Lee 1966a, 1966b, 1966c, 1967a, 1967b, 1967c, 1968a, 1968b, 1969, 1970, 1971, 1973, 1974a, 1974b, 1979c, 1979d).

Allow me to digress briefly about the strange feeling I had at that time as a French European in the province of Quebec, since this illustrates something about the development of Quebec archaeology. At that time, the Quebec amateur archaeological societies relied on English Canadian or American scholars to supervise their excavations and help prepare their reports. At Laval University, French was the teaching language for nearly everyone except Lee, who lectured in English. French was the dominant language in Quebec City, but in Montreal west of Bleury Street, English was usually the only language accepted in stores and public offices. If one dared to address a storekeeper in French, he was, at best, met with a polite and steady, "Would you please speak English!"² The University of

Montreal's Department of Anthropology offered courses on Old World archaeology and Central America but none on North America and the Arctic. In the territory of New Quebec, the newly installed Direction Générale du Nouveau Québec had to compete with the old, established federal Ministry of Indian Affairs (Hamelin 1975: 259). After arriving in Fort Chimo, one was required to obtain a permit from both administrations, which pretended to ignore each other's existence. As a French Canadian, one felt rather undesirable in this northern part of the province. In the small villages, the provincial schools that were built to teach French and Inuktitut were large and luxurious but empty, but the federal schools were old, modest, and full of pupils learning English. Very few adult Inuit at that time spoke English, but even fewer spoke French. The federal government, the Hudson Bay Company, and the English Canadians represented an enduring security for many Inuit, who had bad memories of the French Révillon Frères Company's departure in 1936. And to complete the picture, being an archaeologist, let alone from Quebec, one was regarded as a strange and somewhat suspicious novelty, a kind of miserable tourist with no serious purpose. Archaeologists could not compare with the rich fishing parties, the geological surveyors or mining company experts. Nevertheless, in Fort Chimo, some old settlers were interested in archaeology and served as valuable informants.

But, I would like to continue the history. The elders and near-elders are certainly familiar with Thomas Lee, who had been in conflict with nearly the entire archaeological establishment through various publications (Lee 1970, 1974, 1979a, 1979b, 1980) and in his journal *Anthropological Journal of Canada*. Nevertheless, I owe to Tom Lee my first experience in Ungava archaeology and one of my major research subjects: the Dorset longhouse (Plumet 1969). If I had not been familiar with loneliness and barren landscapes, I would have been discouraged by Lee's hermit-like way of life. But my French cooking made him far more unhappy than I did. For psychological reasons, Lee, equivocally



15.3/ Thomas Lee with Willie Thomasie on the way to Pamiok in 1966

supported by Rousseau, needed to be in aggressive and provocative opposition to the archaeological establishment and its interpretations. To escape damaging and embarrassing disputes, most archaeologists preferred to ignore his attacks and minimize discussion of his Norse interpretations. But by doing so, they have underrated his real contribution to Ungava prehistory. With his proud anxiety and need to distinguish his standards, he published more accurate site descriptions, drew more detailed maps of structures and settlements, and excavated more carefully and slowly, and with deeper (even if biased) thought, than many other archaeologists working in the Eastern Arctic during the 1960s. Thus, I may confirm, after having re-excavated it myself under Lee's direction, that the Imaha Dorset burial is actually an Inuit burial placed inside a Dorset "heavy tent ring" that reused its large stones (cf. Lee 1968 *et seq.*).

In concluding my discussion of these two very honest, but very peculiar characters, I want to say that



15.4/ *Imaha II Dorset longhouse in 1966, before excavation*

I will never forget the deep bitterness expressed by Lee and suggested by Rousseau. The two of them helped to put the archaeology of Arctic Quebec on track, perhaps as a way to compensate for their difficulties. Their endeavors fit perfectly, if unintentionally, with the nationalist feelings growing among French Canadians during this period. But the way that Lee and Rousseau reacted, through their specific personalities, to their conflict with the National Museum of Man contributed to the isolation of Ungava archaeology from the mainstream of normal scientific discussion.

Although I would have been delighted to excavate a Viking longhouse in Ungava, I was not convinced by Lee's argument and thought it was necessary to do more exploration of, and comparison with, other Dorset and Norse houses. I appreciated the opportunity that Hamelin offered me to begin my own research in 1967. My choice of the eastern coast of Ungava Bay, Killiniq, and northern Labrador was influenced by Rousseau and by my desire to avoid any interference with Lee's research area. I also wanted to check the direction of the Paleoeskimo peopling of Ungava Bay and the possibility of Norse remains along this coast. My first attempt, assisted by Gérard Cordeau, was not very successful because of logistical and weather reasons, as well as the carelessness of the transport company: a pilot abandoned our food boxes on a beach at low tide, leaving us to expect it on each

incoming plane! We only learned what had happened two months later.

Thanks to the Inuit from Port Burwell and to the hospitality of the Newfoundlander who was in charge of the post, I was able to do some survey work on Button Is-

land (fig. 15.6), around Killinek Island, in McLellan Strait, and along the Labrador coast as far south as Eclipse Harbor. I even succeeded in traveling in a river canoe south along the eastern coast of Ungava Bay, where I experienced the same difficulties as those encountered 150 years earlier by the Moravian missionaries, Kohlmeister and Kmoch (Kohlmeister and Kmoch 1814; Plumet and Gangloff 1990). We discovered Dorset, Thule, and Labrador Inuit sites and tested one site on Jackson Island. At the time, I was unaware of the significance of these incursions in Newfoundland territory and only later understood the history of frontier problems between Quebec and Newfoundland and the deep resentment of the Quebecois for amputating part of Quebec's territory by order of the Privy Council in 1927 (Privy Council 1927; Dorion 1963; Hamelin 1975: 263; Quebec Government 1971).

As a result of CEN's expansion of activities in the North, French Canadians, along with international scientists from several fields, became involved in Arctic Quebec research. This research was conducted under the authority of the Center, which had its own logistics bases in the north and its own publication series. One of its main projects—the Hudsonie Project—was headed by Hamelin and André Cailleux, a well-known French geologist (Hamelin and Cailleux 1968). Around this same time, the Quebec government's political and administrative presence became more visible and more

effective. French Canadian scholars felt more at home in northern Quebec, and interdisciplinary collaborations could be undertaken with better logistical support.

From 1968 on, Lee concentrated on excavating and reconstructing the Pamiok longhouses (fig. 15.5) and on exploring the western coast of Ungava Bay. The period between 1968 and 1972 corresponds to the climax of the northern Quebec “Viking saga.” In 1968, I submitted a research program to the CEN to undertake explorations north of Lee’s work area and along Hudson Strait. I completed this program in 1968 and 1970, taking into account information collected by the French ethnologist Bernard Saladin d’Anglure, who had just discovered in the Wakeham area the only Dorset petroglyphs ever known (Saladin d’Anglure 1962, 1963). An anecdote illustrates how marginal archaeology still was in Quebec in 1968. I had planned to work in the Wakeham Bay area, but upon my arrival in Fort Chimo, I learned that another French Canadian archaeologist, Georges Barré, a University of Montreal graduate student, was there for the same purpose. Fortunately, northwestern Ungava Bay and Diana Bay were so rich archaeologically that I did not have to compete with Barré, who did excellent preliminary work (Barré 1970).

By the end of the summer of 1968, a general survey of Arctic Quebec had been completed in the east and northwest coasts of Ungava Bay, around the mouths of the George and Payne Rivers, Diana Bay, Joy Bay, the Salluit and Ivujivik areas, and Povungnituk (Wallrath 1958). Inland, Lee had explored the Fort Chimo vicinity, the Lower Payne River and the east end of Payne Lake. Lee and I had started excavations on two Dorset sites with longhouses: Pamiok by Lee and Diana Island by myself. The western part of Ungava Bay as well as Diana Bay were especially rich in Paleoeskimo sites whereas the area near Wakeham yielded more abundant Thule and Neoeskimo sites.

In 1969, when members of the CEN’s Hudsonie Project along the eastern coast of Hudson Bay became puzzled by some structures in boulder fields near Poste-de-la-Baleine, which could not be explained by natural causes, Hamelin asked me to determine if they were anthropogenic. After a brief survey, I identified three Paleoeskimo sites between 50 and 100 meters above sea level. As strange as the choice of a boulder field for a village site may seem to us, the Pre-Dorset people had selected a well-drained location for their settlement. The BAL-1 house structures and artifacts attested to an early Pre-Dorset occupation that was farther south than any others then known. Their location could be explained by a paleoenvironmental reconstruction made possible through interdisciplinary collaboration (Plumet 1976, 1980). Subsequent surveys of other boulder fields in Arctic Quebec revealed many other supposed Paleoeskimo sites, similar to those near Poste-de-la-Baleine, as high as 140 meters above sea level (I. Badgley, personal communication; Gendron and Pinard 2000; Gosselin et al. 1974).

The end of this initial period of research in Arctic Quebec was characterized by large survey efforts that were conducted with very poor logistical and financial support. The meager funding available ranged from \$1000 to \$4000 for two months of fieldwork, with almost no salary for assistants and no funds for analysis. There was no specific program to finance



15.5/ *Imaha II* after reconstruction by Lee in 1968

archaeological research and, except for the CEN at Laval University, where archaeology was marginal to the university's research program, no academic department was seriously involved in Northern or Amerindian archaeology.

Quebec Period

In Southern Quebec, tremendous changes occurred in social and political life during the late 1960s. As a graduate student in France, returning to Montreal every summer between 1966 and 1969 before flying north for fieldwork, I was probably in a better position to perceive the changes than if I had stayed in Quebec continuously. In addition to evolution of self-affirmation that began with the so-called Quiet Revolution, a number of external or accidental events accelerated this movement: the Montreal Exposition of 1967, which awakened many people in Quebec to international affairs, especially to the world's cultures and their heritage; the 1968 political movement that helped acquaint local activists with those of other countries; and the drift away from a "Quiet Revolution" to a less quiet, less folkloric, but more spectacular and effective action. The elders and near-elders will remember the events that punctuated the usual quietness of Quebec and of Canada between 1967 and 1970. They culminated in October 1970 with a double kidnapping and the death of Pierre Laporte, a minister in the Quebec government. By European standards, these October days appeared amateurish and unimpressive, but by Canadian standards, they embodied both the bad and good results of a real revolution. Even before 1970, there was a perceptible increase in self-confidence among Quebec citizens and, west of Bleury Street in Montreal, a decrease in the arrogance of store keepers and bank clerks who started to answer in French or flatly apologized if they could not! This was not merely anecdotal but symbolically important because the ability to use one's own national language in one's own country is culturally and socially vital for a people.

For archaeology, the changes induced by these political and social transformations were significant. In 1969, inspired by the 1968 political movements, the University of Quebec was created. One of its goals was to encourage scientific and cultural development in Quebec society. My colleague, Gilles Tassé, and I were appointed to the Montreal campus of this university specifically to create a program in Amerindian and Eskimo archaeology. Although a few archaeology courses had been taught as early as 1963 at the University of Montreal, these courses had been oriented primarily toward Mexico and the Old World. In the new program, even officers of the National Museum of Man in Ottawa were invited to give courses in Amerindian prehistory. At the end, with pressure from students, volunteers, and nationalist groups, the Department of Anthropology became involved in Amerindian archaeology. In another change, the Ministry of Education, in addition to granting scholarships, began to offer specific programs to subsidize academic research in the humanities, including archaeology. In 1970, the archaeology of North America, in addition to many other new subjects, gained official academic status in Quebec. This was a year of optimism, but also of illusions. While it was evident that we had to work in French, the limited tradition of archaeological training in Quebec was in English. The European French language could not be imported *tel quel* to transmit the concept of North American archaeology because it would have been perceived in Quebec like a different form of imperialism. Mere translation was meaningless; the insertion of American words and concepts in French sentences would have resulted in a "creolization" of the archaeological literature (Plumet 1987). Certainly at that time it was easier and more rewarding for one's career to publish in English. One can retrace—with a sense of humor—the inconsistencies and errors in the first publications on Quebec archaeology in French. Once again, the elders and near-elders will recall the 1970 Ottawa Canadian Archaeological Association meeting when a small group of

young Quebec archaeologists, mostly students, announced the creation of their own association, Association pour la Recherche Archéologique au Québec (ARAQ), and their intention to “archaeologize” in French.³

In 1971, the periodical *Recherches Amérindiennes au Québec* was created, followed in 1974 by the University of Quebec in Montreal’s monograph series *Paléo-Québec* to facilitate the diffusion of research results in French. In 1977, the first issue of *Études Inuit Studies* was published at Laval University. The former was devoted to anthropology, including Amerindian and Eskimo archaeology; the second published paleoenvironmental sciences, archaeology, and ethnohistory; and the latter specialized in Arctic anthropology and archaeology. At the end of the seventies, *Recherches Amérindiennes au Québec* started its own monograph series. Even today, most research in southern and northern Quebec archaeology is disseminated in these publications and in those of the Quebec Ministry of Cultural Affairs. In addition, the *Paléo-Québec* series, which focused on arctic regions, began publishing summaries in Inuktitut. Since 1993, this series has been issued by *Recherches Amérindiennes au Québec*.

In the climate of Quebec at the dawn of the seventies, I must say that Elmer Harp’s 1967 to 1975 expeditions to Arctic Quebec and the Belcher Islands seemed more of a continuation of the previous era than the beginning of a new one. In 1970, with the help of the Ministry of Cultural Affairs, I tried to connect Harp with some Quebec students who would have benefitted from his experience, and in 1972 I organized a joint field program between Jean-Paul Salaün and André Gosselin with the CEN project in Guillaume Delisle Lake, previously known as Richmond Gulf. For the first time, a multi-disciplinary French Canadian scientific expedition, numbering more than fifteen persons, worked in

nearly the same area as an American team. While no coordination was undertaken at that time, during 1972 a new law was passed on the Cultural Heritage of Quebec, and, after 1974, a research permit was required to perform archaeological fieldwork. As for my work, I greatly appreciated the open exchange and sharing of data I had with Elmer Harp, which improved my 1976 and 1979 monographs on the Poste-de-la-Baleine (now Kuujjuarapik) Pre-Dorset sites.

These field projects first established the presence, along this stretch of the east coast of Hudson Bay, of Paleoeskimo occupation, including early Pre-Dorset components and an undated Dorset component at Great Whale; a very late Dorset component, which included a Norse copper pendant, at the mouth of Guillaume-Delisle Lake (Harp 1974–1975); and several unusually large Paleoeskimo mid-passage structures in the northern part of the lake (Gosselin et al. 1974). In the Belcher Islands, Elmer Harp found a complete sequence of raised beaches with Paleoeskimo settlements that was comparable, although less extensive, than that found by Jørgen Meldgaard at Igloodik. The Belcher sites could have had a great impact on arctic archaeology, but like the Igloodik sequence, it remains only partly published (Harp 1997). During the time when Harp worked in the Belchers, José Benmouyal analyz-



15.6/ The crew at MacColl Island, one of the Button Islands, in 1967.

ed Desgloffe's collections for his master's thesis and established that the "Manitounik" culture did not exist, but was simply a mixture of tools from several different components (Benmouyal 1978).

Between 1970 and 1980, the Quebec government greatly increased its involvement and power in Arctic Quebec and initiated cooperation with the Inuit. With these developments, the conditions of archaeological research in Arctic Quebec improved. In 1970, 1973, and 1974, I conducted research on northwestern Ungava Bay, Akpatok Island, and Diana Bay. I was mainly interested in studying Paleoeskimo settlement patterns and incorporating a paleoethnographic approach to Dorset houses and habitat. In France, I had been trained by André Leroi-Gourhan in this approach, which had not yet been employed in the American Arctic; I had also seen how much information had resulted from the careful excavations conducted by Lee. According to publications available in 1970, no longhouses, aside from the six known between Payne River and Diana Island, had been found anywhere in the Arctic except at established Norse sites. If the unique, dramatic, and elaborate structures discovered at Ungava were indeed Dorset houses, they would have to be interpreted, not from an Iron-Age Norse base, but from the technological and socioeconomic contexts of a seminomadic Arctic hunting society. A comparison between different types of habitations and different sites was necessary. In my 1969 monograph, I assumed that more longhouses might be found in the Canadian Arctic and Greenland if archaeologists were prepared to look for them along with the usual small dwellings. Thirty years later, in 2000, more than forty such structures have been found between eastern Victoria Island and northeastern Greenland. In that volume, I also suggested that multidisciplinary studies were needed to investigate cultural responses to paleoenvironmental fluctuation (Plumet 1969:56).

In Quebec in the early seventies, a major difficulty was assembling an archaeological team. Except for a small group of specialized scholars, Arctic Quebec was

outside the field-of-consciousness of most Quebecois. In 1970, I retained the services of a young French navigator, and, at the last moment, found a design student in my university. Both were attracted to the project by arctic adventure, but none was an archaeologist. They turned out to be excellent assistants and enjoyed their two-and-a-half months of difficult fieldwork, mainly at the Qilalugarsiuviik longhouse. In 1973, at Diana Island, I engaged a multidisciplinary team that included: palynologist Pierre Richard; a geomorphologist who abandoned the project when he returned south; one technical assistant from Quebec; and Jean-Paul Salaün, a French student whose master's thesis dealt with Thule house structures (Salaün 1972). In 1974, after our Akpatok survey, Salaün and three assistants from Quebec went to Diana Island during August but, because of stormy weather, were unable to work more than five full days. After they returned to Montreal, we reconsidered our logistics strategy, along with the high cost of individual expeditions (around \$15,000), in light of the area's little-known but apparently rich prehistory. In order to combine a multi-disciplinary program with a broader regional perspective and a safer logistics environment, we decided to apply for a five-year project to a new program of the Canadian Humanities Council. The result was the Tuvaaluk (the Inuit name for Diana Bay) project, which was endowed with nearly one million dollars. It was designed to enable academic scholars in anthropology and the environmental sciences to cooperate according to the specific needs of their discipline, but with archaeological research as the primary goal. Our team of eight experienced specialists, who were eager to work together, attracted students to arctic research and promoted the development of archaeology at the University of Quebec at Montreal (Plumet 1978). Of the eight scholars, seven of whom were relatively recent immigrants from Europe, four were at the University of Quebec, three in Montreal, and one at Laval University in Quebec. I remember that because of the strongly nationalistic feelings in Quebec at the time and the

new Canadian nationalism, we wondered if so many Neo-Canadians might be a handicap for acceptance of the project; but when the question was put to the federal officer in charge of the grant program, he answered that it would only be a departure from the many American teams working in Canada.

At the same time that

Tuvaaluk started, in 1975-1976, the Quebec government's James Bay project began a large archaeological contract program in the forest region. This project created a strong demand for archaeologists at a time when Quebec had only undergraduate students available. For this reason the James Bay and Tuvaaluk projects contributed, in different ways, to training many of the archaeologists now working in Quebec (fig. 15.7). The complete absence of graduate and postgraduate archaeology students made the Tuvaaluk program less efficient than it could have been. Long, hard strikes at the University slowed the first two years of the project and led to important changes in the archaeological team. Ian Badgley, a graduate student from the University of Toronto, assisted me during the last three years of Tuvaaluk fieldwork, and his skill in stratigraphy was much appreciated.

Even though the main part of excavation activity was concentrated in northwestern Ungava Bay and Diana Island, the Tuvaaluk project covered the broader region extending from Labrador to the south shore of Hudson Strait and included inland surveys to Payne, Klotz, and Robert Lakes. It also permitted a stimulating collaboration with the Smithsonian research program in northern Labrador, which was conducted by William Fitzhugh. The sharing of data between these two projects lead to several publications (Archambault 1981; Boutray 1981; Plumet 1981b; Plumet and



15.7/ Tuvaaluk team in 1978, outside the large wood cabin built in 1976 to shelter scholars and students

Gangloff 1990) and resulted in research and salvage efforts at the Nunaingok site on McLelan Strait, in which logistical support was provided by Tuvaaluk and funding by the Quebec Ministry of Cultural Affairs. Because of the lack of trained archaeologists from Quebec, Badgley suggested that Henry Stewart, an Ph.D. student from the University of Toronto, be put in charge of the fieldwork under Tuvaaluk direction, assisted by three undergraduate French Canadian students.⁴

At the end of the Quebec Period in the early 1980s, the rapid development of contract archaeology in Quebec drew most of the archaeology students. More than twelve newly established private companies provided short-term but higher income jobs to the few graduate students and even to undergraduates than academic institutions could offer. Working as a research assistant in an academic program was no longer attractive, except to the few who were deeply scientifically minded and were ready to accept long-term training and low incomes. This situation, peculiar to archaeology, posed a major problem for the exploitation of the rich trove of Tuvaaluk archaeological data.

The results of the Tuvaaluk Project have been published in eight monographs, forty papers, and four master's theses (Bibeau 1984; Derosiers 1982; Gauvin 1990; Labrèche 1984; Laboratoire d'archéologie 1993). By 1980, more than 350 sites had been registered in Quebec territory for the Inuit area whereas only forty

probably felt that it was easier to negotiate the symbolic value of the archaeological heritage than the economic problems of Nunaviks' or New Quebec's rich mineral resources. Thus, as it had during Quebec's nationalist movements in the early seventies, archaeology became a symbol of Inuit "ethnicity" (Plumet 1979a). When the Tuvaaluk team landed at Quaqtaq in June 1978, it was greeted—with the Inuit's sharp sense of humor—by posters that claimed that only English and Inuktitut could be spoken in Inuit land! After that, we had to negotiate each field project with the Inuit municipality, but this was not a real problem.

In the early eighties, senior Tuvaaluk Project scholars took time to organize their field data and to present it in various theses and publications. For the archaeological fieldwork, this pause was welcomed. It permitted the Inuit and the Quebec government to establish a new deal. Several assistants left for contract archaeology or to continue graduate work at other universities, since the University of Quebec in Montreal did not offer a graduate program in archaeology. Badgley was hired by Avataq to take charge of archaeology in Arctic Quebec in compliance with regulations issued by the New Quebec section of the Ministry of Cultural Affairs archaeological service.

The sudden interest in archaeology expressed by the Inuit was a welcome development. We heard that an archaeology field school would be offered, as well as new research programs following Inuit priorities. This development was similar to what had happened in southern Quebec in the seventies. It could have provided an excellent opportunity to develop new collaborative efforts between academic scholars and students and the Inuit interested in their arctic heritage. The Tuvaaluk Project offered an excellent training program, but although I made suggestions to this effect to the Ministry of Cultural Affairs and to Avataq, quite different choices were made.

From 1980 to the end of the 1990s, so little information has been published that it is difficult to follow the development of archaeology in Arctic Quebec.

During this time, the New Quebec section of the Ministry of Cultural Affairs and Avataq began a new archaeological project without any consultation with the university. The unpublished Tuvaaluk data, however, seems to have played a role in the project's establishment, and I was happy to hear from Badgley that one of the most important Paleoeskimo sites, located near Quaqtaq, had been the subject of an Avataq archaeological field school (it was a salvage project). According to Badgley, the site contains a large Groswater component that may be one of the richest for this cultural period in the Eastern Arctic. The site has completely disappeared with the extension of the Quaqtaq landing strip. Another discovery that might influence the interpretation of the Tuvaaluk data seems to have been made by Dave Okpik, of Quaqtaq, who located, near the southeastern bottom of Diana Bay several kilometers from the shore, the source of what was called Diana quartzite during the Tuvaaluk project. This raw material, which initially was mistaken for Ramah quartzite (i.e., Ramah chert), was exploited as early as the beginning of the Paleoeskimo period (Boutray 1981; Plumet 1981b, 1986b). According to Badgley (personal communication), the quarry is associated with an important Dorset settlement and with caribou hunting. In 1987 and 1988 Henry Stewart, then a teacher in a Japanese university, was put in charge of a new excavation project at the Nunaingok site on McLellan Strait. This important project seems to have been funded by the Ministry of Cultural Affairs through Avataq, but very little information of any kind about this project has been made available.

Despite the Avataq surveys, it is contract archaeology that has dominated northern Quebec archaeology during the Inuit period. From 1984 to 1992, these projects drained at least \$1.6 million from the Ministry of Transport and the Ministry of Cultural Affairs. Based on what Badgley let me see of the collections, and from unpublished preliminary reports deposited at the Ministry of Cultural Affairs, very promising discoveries have been made in various locales of Arctic Quebec. In

1989 alone, 432 sites were registered in the Quebec database.⁶

We may wonder if the absence of scientific publication during this period is due to the fact that few academically trained archaeologists are involved in research in Arctic Quebec. The Avataq team and private firms have few graduate students and no Ph.D. archaeologists on their staffs, although some workers have long and serious experience. Two exceptions are worth mentioning. Yves Labrèche, an early assistant in Tuvaaluk and by this time a Ph.D. student at the University of Montreal, received a small grant from the Quebec Ministry of Cultural Affairs and the Secretary for Autochthonous Affairs, through the University of Quebec in Montreal, to carry on limited ethnoarchaeological research with the Inuit around Kangisujuaq from 1985 to 1989 (Labrèche 1984, 1986b, 1988, 1991). In addition, Murielle Nagy of the University of Alberta studied the Pre-Dorset-Dorset transition in collaboration with the Inuit of Ivujivik (Nagy 1992a, 1992b, 2000a, 2000b). Both of them made some of their results available and published papers. Fortunately, their work has helped keep individual academic research alive in the Arctic Quebec.

Other than these exceptions, as far as I know in 1993, no post-graduate work has been done by Quebec students in Arctic Quebec since the beginning of the Inuit period. Specialized teaching in arctic prehistory is totally absent in French Canadian universities, although it has existed since 1988 at McGill University, where Ph.D. theses dealing with Thule subsistence were defended in 1992 and 1999, and three master's theses on the Canadian Arctic are under preparation. In more than thirty years since 1961, when the University of Montreal's Department of Anthropology was created, only two Ph.D. theses have dealt with the archaeology of northeastern North America. The last one was Marie-France Archambault's study of the Archaic of the mid-North Coast of St. Lawrence River (1993). None have focused on the Arctic.

Certainly, the choices made by Avataq and the New Quebec section of the Ministry of Cultural Affairs have not encouraged academic scholarship in Arctic Quebec. By contrast, the approach taken by the North Coast of the St. Lawrence section of this ministry has been quite different. In the eastern portions of the North Coast, Paleoeskimo and Neoeskimo research has recently been conducted by Quebecois. This research has not only produced interesting scientific results but it has also involved the community's collaboration in archaeological heritage.⁷ Another perspective, however, is possible. In Arctic Quebec, the cultural and archaeological heritage is directly linked to the Inuit population, for which Avataq took responsibility, at first in compliance with the sovereignist government of Quebec. This heritage is largely alien to French Quebec culture, but its rapid, successful, and peaceful takeover by the Inuit, as well as the increasing trend toward self-rule by Inuit throughout the entire North American Arctic, is in contrast, at least in a symbolic sense, with the slow, conflict-ridden, and halting progress of Quebec society toward its own autonomy and cultural affirmation.⁸ In southern Quebec, as in southern Canada, the relationship with the Amerindians and their heritage is different. Amerindian communities had a history that was much more deeply linked with that of Quebec. In April 1993, *Le Devoir* reported that during the last five years the number of Quebecois—as well as Canadians—who asserted that they had an Amerindian ancestor increased by 70 percent (a total of 137,615 individuals in Quebec and more than one million in Canada). Today, Quebec archaeology is strongly oriented toward Amerindian archaeology, especially the contact and historic periods. Arctic archaeology has perhaps been more attractive for New Quebecers like myself who may easily separate their research goals from issues of individual or cultural identity.⁹ Meanwhile, the French language is becoming more commonly used among the Quebec Inuit, whose official publications are trilingual.

1993 to 2002

Thanks to a special agreement with my university, which allowed me to use the last three years before an early retirement to work almost exclusively on the Tuvaaluk results, I have been able to make more data available. In a provisional synthesis of the Diana Bay prehistory, which was published in a volume honoring William Taylor (Plumet 1994), I have critiqued Park's argument for the noncontemporaneity of Dorset and Thule cultures. Since Park seems to have dismissed my claims in later papers (Park 1993, 2000), this interesting problem remains unsettled. Because of the expense and time involved in traditional paper publication, especially without funding, I have also placed on two websites all of the project results available in 2000: reports of various Tuvaaluk surveys; and detailed field-work reports at the Cordeau (JfEl-1) and Tuvaaluk (JfEl-4) sites, with maps, plans, photographs, and diagrams. I have emphasized the stratigraphy and structural organization of the Tuvaaluk site House A, "a Dorset-Thule house," and the occupational sequence in this complex settlement and have presented different interpretations of the types of Dorset-Thule relationships that might be represented at this strange house (fig. 15.8). The Tuvaaluk website has been updated as of May 2002.¹⁰ A second website, Nunavik, went live in May 2002. In addition, I have concluded my work with Serge Lebel on the technological study of the Dorset tip-fluted points, using the same approach that we employed for the Dorset metabasalt core industry (Lebel and Plumet 1991; Plumet and Lebel 1991, 1997).

In Nunavik, the Avataq surveys continued through the 1990s, and some of the archaeological work from the previous twenty-five years was finally published. This was precipitated by the awareness of my Danish colleagues that the Dorset petroglyphs discovered at Qajartalik near Kangirsujuaq (Wakeham Bay) by Saladin d'Anglure in 1961 were

endangered. Tourists from cruise ships landed on the unprotected site without authorization. Hans Kapel offered to organize a joint salvage expedition with Quebec scholars and a multidisciplinary team of trained specialists in arctic archaeology and rock art studies, along with Greenlandic archaeology students and some Nunavik Inuit. The project would have also established a museum or interpretation center in Kangirsujuaq. In 1994, I transmitted this fascinating proposal to Daniel Gendron, the undergraduate student in charge of archaeology at Avataq. Without declining the proposal, Avataq organized in 1996 its own small-scale expedition to Qajartalik, led by Daniel Arsenault, a young rock art specialist from Quebec who was trained in England. At last, new research and salvage work has begun at this unique Dorset site, revealing new figures previously hidden under the soil (Arsenault et al. 1998; Plumet 1997). Two new master's theses have been accepted at the University of Montreal on the analysis of small Paleoeskimo sites and collections from Hudson Bay (Bernier 1997; Pinard 2000). Recently, Gendron



15.8/ *The "Dorset-Thule House" at Tuvaaluk which contained Dorset-style features such as an axial hearth and Thule-style features such as a cold-trap and fireplace.*

published a comprehensive paper on pre-Dorset boulder field structures found in Nunavik since 1969 (Gendron 2001). Of a more fundamental nature is the research initiated by Murielle Nagy while a graduate student at the University of Alberta, Edmonton. Using data from her own excavations of sites in the Ivujivik area, she reevaluated the Pre-Dorset–Dorset transition that was first established by Taylor in 1968 (Nagy 1994a, 2000a, 2000b). Finally, at the turn of this century, Avataq archaeologists published the first overviews of Early Paleoeskimo occupation in Nunavik, and of the Dorset occupation on the south shore of Hudson Strait, using the entire set of data collected for this period since 1969 (Gendron and Pinard 2000, Pinard 2001). According to this overview, the two distinct Paleoeskimo populations—one from Labrador and the other from Foxe Basin—whose existence I had hypothesized from the Tuvaaluk data at the end of the 1970s seems more significant than ever.

Conclusions

The history of archaeology in Arctic Quebec needs to be considered in its widest sociological and political context. These events offer interesting perspectives on the troublesome and partly unconscious relationships between English and French Canadians, between Old and New Quebecers, and between Inuit and other Canadians. But it should not be forgotten that Nunavik is a rich and underexploited archaeological area where some of the most important Neo-eskimo and Paleoeskimo period sites are found at large interior lakes (Payne, Klotz, and Robert's) at some distance from the coast. The distribution and size of Paleoeskimo sites from the earliest phases underscores the inadequacy of the old single “core area” concept. New excavations of Dorset longhouses (of which more than fifteen have now been located around the western peninsula) could shed new light on Dorset settlement patterns, especially if they were compared with the more than forty longhouses discovered during the last twenty years in the Canadian Arctic archipelago,

Ellesmere, and even northwestern Greenland. Among the many questions raised in Quebec Arctic archaeology, one of the most interesting is the nature of the relationship between very Late Dorset communities and Early Thule ones. Tuvaaluk House A, which is half-Dorset and half-Thule, contradicts Park's speculations on this question. I hope the next generation of archaeologists, Inuit as well as Euroamerican—and why not European and Asian—will rid themselves of nationalistic complexes and work together toward the clarification of this fascinating part of world prehistory.

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I would like to thank all the persons who gave me information. I am especially grateful to William Fitzhugh, Louis-Edmond Hamelin, Roger Marois, Jean-François Moreau, Jérôme Rousseau, Gilles Tassé, and William Taylor, who made valuable comments or corrections on earlier drafts of this paper, for which, nevertheless, I am the only one to be held accountable.

Notes

1. It will be the task of historians to settle the truth from among the disparate and often confusing explanations of these conflicts. A recently published essay (Laporte 1995) is apparently an unbiased first step toward this goal, and it reads like a thriller. According to Laporte and other sources I used while preparing this paper, which included former members of the National Museum and Jacques Rousseau's son Jérôme Rousseau, now an anthropologist at McGill University, the following picture can be tentatively drawn. Contrary to sometimes too readily accepted opinions in Quebec, these conflicts cannot be reduced to antagonisms between English and French Canadians, since the main clashes were with Marius Barbeau and Marcel Rioux, whose dogmatic Marxist tendencies, according to some informants, did not please Jacques Rousseau. (However, I must note that Jérôme Rousseau disagrees on this last point.) The explosive and highly publicized aspects of these conflicts may partly have resulted from Rousseau's stringent, scientific, but very direct and free-spirited temper that contrasted culturally with the more mild-mannered English style. In any case,

various circumstances and facts seem to have converged to create a dramatic outcome. Rousseau's appointment to the directorship of the National Museum by Prime Minister Louis Saint-Laurent had not followed the customary procedures of the day. In particular, Rousseau had explicitly refused to become a member either of the Society of Freemasons or of the *Ordre de Jacques Cartier*, a comparable French Canadian secret society, as his colleagues of similar standing had customarily done. Rousseau then apparently tried to reorganize, and also to moralize, some functional aspects of the museum, which had been plagued by sexual harassment of secretaries. As Laporte clearly explains, however, the Long-Sault affair became the central issue. Marius Barbeau, who had been convinced of Lee's discovery from the start, was obliged to change sides under pressure from the Freemasons, of which he was a member. He advised his newly appointed friend Rousseau to get rid of Lee who, meanwhile, had aggravated his own professional standing by claiming a too-ancient age for the Sheguindah site on Manitoulin Island. In those days, more than today, a claim of 30,000 years B.P. was not scientifically correct. But Rousseau, who was neither politically nor ideologically involved, settled the Long-Sault case from a purely scientific point of view. After a careful examination of the data and of the Hawkesbury site, he came to Lee's defense. To render things still more complicated, officials from Ottawa, as well as French Canadians under the leadership of the famous clerical nationalist historian Lionel Groulx, fought fiercely against Lee's claim for the site, which happened to be on the Ontario side of the Ottawa River. French Canadians wanted to celebrate at Carillon, on the Quebec side, the battle where their hero Dollard des Ormeaux was killed together with seventeen young men. At last, John Diefenbaker, who is not known for his amity toward French Canadians, replaced Saint-Laurent as Prime Minister, and a bill was specially prepared and passed to revoke the appointment held by the troublesome Jacques Rousseau, who had to vacate his position at the National Museum.

2. Even French Canadian clerks and storekeepers objected to the use of French. In some rare circumstances, in the Montreal far west, I have been snubbed by a contemptuous, "Could you speak White," a word that gave rise to the title of

a famous and subversive book of the sixties: *Nègres blancs d'Amérique* (White Niggers of America) by Pierre Vallières (1968), one of the Independentist leaders. At the end of the seventies, in an Ottawa hospital, a French European archaeologist was surprisingly met with a "Could you speak Christian!" from a female doctor.

3. Unfortunately, very shortly after its foundation, the A.R.A.Q. disappeared. It had been initiated by members of the S.A.P.Q. (Société d'archéologie préhistorique du Québec) apparently to gather together the few archaeologists of Quebec Province. As a matter of fact, it was mainly oriented toward settling a score with a then too-well-known amateur archaeologist and a European teacher at the Department of Anthropology of Montreal University, who later found an interesting position in an American university. These manipulations hurt deeply the archaeological circle of Quebec, which has been left divided since then. More than fifteen years later, when a new association was founded with the appearance of contract archaeology, the A.A.Q. (Association des Archéologues du Québec), in spite of its welcomed realizations, never succeeded in attracting all the archaeologists with the academic ones, in particular, being on their guard.

4. H. Stewart, who returned to Japan in 1979, never completed the final report on this project. The results of the work accomplished at Nunaingok in 1978 are taken from Stewart's preliminary report in Plumet and Gangloff (1990).

5. The reader certainly understands that the term "ghetto" is used in a humorous, ironic sense and in paradoxical contrast to the apparent situation of the French-speaking Quebec Province inside mainly English-speaking North America. It reflects the feeling of many non-American scholars confronting the relative imperviousness of Anglo-American science to non-English scientific literature, as well as the tendency of many Anglo-American scientists to mistrust or ignore foreign publications. It is true that English tends to be the international language in several fields, at least in Europe and in the Commonwealth countries. It is less true in Latin America. As for Russia, China, and Japan, this acculturation is not as evident. There, English is not yet the *lingua franca*, and the most important scientific publications are issued in the national language. Many Japanese archaeologists seem to be more interested in learning Chinese or Russian than English. I believe that for

a scientist, it is as important to publish the main results of one's research in one's own cultural language as to be able to understand several foreign scientific languages.

6. Some information concerning contract archaeology in Arctic Quebec is available in the series *Recherches archéologiques au Québec* published annually by the Association des Archéologues du Québec since 1986 (for the years 1983–84).

7. An interpretive center has been built at Grandes-Bergeronnes, close to two of the sites excavated. See also Martijn(1974), Pintal (1991), and Plumet et al. (1992) for the Blanc Sablon area; Plumet et al. (1993) for Grandes-Bergeronnes; and Archambault (1994) for the middle North Coast of the St. Lawrence River.

8. In contrast with the 1970s and with the slightly increasing use of French by the Inuit during the last period, Quebec archaeologists, as far as the prehistory of Quebec is concerned, tend to

publish and communicate more and more in English. In 1993, the 26th Annual Meeting of the Canadian Archaeological Association was held in Montreal for the first time. It was an exceptional opportunity to valorize the "archéologie québécoise." More than 40 percent of the papers given by French Canadian prehistorians were in English (mostly presented by university teachers and students while those in French were given by independent or public archaeologists) and papers dealing with historic and contact archaeology generally were in French. This trend was still evident in 2000.

9. I expressed my understanding of this problem concerning identity just before the Quebec referendum in *Le Devoir* (Montreal), May 16, 1980, p. 10 (Plumet 1980). A more extensive study may be found in Plumet 1986b.

10. See <http://www.unites.uqam.ca/tuvaaluk> and <http://www.unites.uqam.ca/nunavik>.

The History of Archaeological Research in Nunavik (Nouveau-Québec): A Second Opinion

CHARLES A. MARTIJN

In the spring of 1993, the Elders Conference on the History of Archaeology in the Eastern Arctic was held at Dartmouth College, New Hampshire, to honor Dr. Elmer Harp, Jr., on the occasion of his eightieth birthday and to extend official recognition to other pioneer prehistorians in this field, namely Frederica de Laguna, Eigil Knuth, Father Mary-Rousselière, and Graham Rowley. Among the numerous speakers at this festive occasion were Patrick Plumet, former director of the Laboratoire d'archéologie at the University of Quebec in Montreal, who delivered a paper entitled "History of the Archaeological Research in Arctic Quebec" (Plumet, this volume). His remarks dealt in part with what he termed "the psychological, sociological and political context" of archaeological work in northern Quebec. Sitting in the audience, I was bemused by the tenor of some of his assertions about events and developments in which I myself, since 1958, had been an active participant or a close observer (Martijn 1998). Episodes of this nature lead one to reflect once again on whether there is really such a thing as "history." Perhaps there are only "histories"? This paper offers a second opinion on the history of northern Quebec archaeology, while fully recognizing that it, in turn, reflects my own biases.

It is not my intention here to provide the reader with a comprehensive treatise on the development of archaeological research in Nunavik (Arctic Quebec), or Quebec in general for that matter. A balanced overview would require a team effort so as to reduce



the element of subjectivity to a minimum. Nevertheless, those interested in this topic may consult a number of sources that, despite their shortcomings and a need for in-depth revision, provide a variety of perspectives (e.g., Cinq-Mars and Martijn 1981; Gélinas 2000; Harp 1984; Martijn 1979, 1998; Martijn and Cinq-Mars 1970; Plumet and Gangloff 1990:3-6; Taylor 1964b).¹ That said, the best review and assessment of research problems relating to prehistoric Inuit archaeology in northern Quebec are still, in my opinion, to be found in an unpublished report by Badgley (1984).

Four things in particular struck me about Plumet's paper (this volume): (1) the lack of attention accorded to the cultural and political aspirations of the Inuit communities in Nunavik; (2) the blinkered academic outlook on archaeological heritage; (3) the misinterpretation of Quebec government policy in regard to native prehistory; and (4) the wide divergence in outlook between him and his Quebecois colleagues on the matters above.

Plumet, speaking “as a French European in the province of Quebec,” provides a brief account of the linguistic altercations that marked Montreal during the 1960s. Such tensions, often arising from deplorable incidents, played a key role in fueling the nationalistic upheavals that characterized that period. To what extent they also had a direct impact on the development of local prehistoric research remains uncertain. To me, the roots lie much deeper and are more variegated than those offered by a simple cause-and-effect explanation of linguistic tensions.

Archaeological studies in Quebec have progressed in complex ways over a period of two centuries, and have been subjected to various forces (Gélinas 2000; Martijn 1998; Picard 1979; Trigger 1981). During the course of the nineteenth century, sporadic avocational contributions were made by members of both the French- and English-speaking sectors of the population who, in most instances, rarely coordinated their efforts. On the Quebecois side, interest in archaeology was centered initially on historical remains associated with the French Regime, as part of a movement to provide “French Canadian” society with its own historical self-image, to conserve its cultural heritage, and to promote its nationalistic aspirations. With a few notable exceptions, native ethnohistory and prehistory remained marginal topics to these primary issues and were mostly ignored well into the twentieth century. In the quest to ensure the ethnic survival of Francophone Quebec, the emphasis was placed on “studying ourselves before studying others.”

Paradoxically, it took another wave of nationalistic fervor and self-affirmation, but from a different mold, the so-called Quiet Revolution of the 1960s, to reverse this negligent attitude. A new climate of intellectual ferment, not only inward-looking but also outward-looking, marked by a breakaway from outdated restrictive concepts, promoted more sustained and diversified reflection on the phenomenon of native communities existing within the confines of a larger Euro-Quebecois society. Among other things, it helped to

instill the realization that prehistoric sites formed part of the province’s overall cultural heritage. Some circles expressed concern that the lack of legal protection allowed artifact collections to be removed with impunity by outside institutions and researchers. In the absence of provincial government intervention and academic implication, informed amateurs, increasingly aware of archaeological discoveries being made elsewhere in Canada and the United States, set up regional societies to undertake local explorations. The eventual creation of anthropology departments at several universities and the hiring of professional archaeologists not only ensured a more viable interest in the discipline but also had a secondary effect. Some university students, rather than following their professors to places such as the Mediterranean, the Near East, or Central America, began to insist on opportunities to participate in prehistoric projects within the boundaries of Quebec itself, so as to provide them with a chance to make their own personal contributions to the paleohistory of the Northeast.

Throughout the succeeding decades, Quebecois Amerindianists, including archaeological representatives, engaged in wide-ranging discussions about the cultural, social, economic, and political problems faced by native peoples within Quebec, who were caught between two competing bureaucracies, federal and provincial (Gélinas 2000). Specific attention was also directed to ethical and legal questions regarding the obligations of researchers toward native communities and the participation of native persons in anthropological projects of all sorts. As I can testify myself, this was done with the kind of sensitivity and awareness that springs from a firsthand familiarity with, and a long-time reflection about, minority status.

Plumet remained aloof from these proceedings with one exception. Apparently apprehensive about possible restrictions on his archaeological activities, he argued against the ownership of archaeological remains by First Nations minorities (Plumet 1979a, 1984, 1986a). In his opinion, such cultural resources should

be classed as world heritage and presumably managed by nation-states. These views were only expressed in academic publications that, for all practical purposes, are inaccessible to northern communities, leaving them unaware of such lobbying efforts. It does not appear that he has ever discussed this view with native representatives to obtain their reaction to this. Unlike other colleagues in northern Quebec, during the more than twenty years of his career as an arctic archaeologist, Plumet never hired a single Inuk for his excavation crews, nor employed one in his laboratory. The ones who worked for him did so as boat owners for transportation purposes, as food providers, or as informants. He states that, "I tried from 1976 on to interest Inuit in archaeological research, but slide projections in the villages and visits to the excavations seemed to be of little interest to them." Other archaeologists possibly had more success with such measures—one does what one can and not everyone is a born communicator—but in the long run it is clear that different and more sustained methods need to be employed. Plumet did make it a point to always include a summary in Inuktitut for each number of his *Paléo-Québec* publication series. From an academic point of view, such a gesture is certainly laudable, but from a practical point of view, at the community level, experience has shown this to be totally ineffective.

Transposing the French-English linguistic imbroglio to the Far North, Plumet reformulates it in an archaeological context. He explains:

While it was evident that we had to work in French, the limited tradition of archaeological training in Quebec was in English. The European French language could not be imported *tel quel* to transmit the concept of North American archaeology because it would have been perceived in Quebec like a different form of imperialism. Mere translation was meaningless; the insertion of American words and concepts in French sentences would have resulted in a "creolization" of the archaeological literature. (Plumet, p. 194, this volume)

These are valid preoccupations from an academic point

of view, but one wonders, for example, if Nunavik archaeology, or arctic prehistory in general, might not benefit from a study of Inuktitut terms for artifact types and for Inuit categories and concepts relating to their utilization. One imagines, too, that Inuit students would welcome a lexicon in Inuktitut, French, and English that dealt with traditional cultural equipment, habitation features, and other relevant elements related to prehistoric remains, compiled with the assistance of community elders. This is an approach that the Avataq Cultural Institute has applied in northern Quebec field situations for a number of years now.

Plumet (this volume) divides his history of archaeological research within Nunavik into three periods: the International Pioneering Period, lasting until the end of the 1960s; the Quebec Period during the 1970s; and the Inuit Period from the 1980s onward. In his discussion devoted to what is termed the Inuit Period, Plumet fails to make any reference to a remarkable personage, a young Inuk from the village of Inukjuak named Daniel Weetaluktuk, who possessed a veritable passion for archaeological research (fig. 16.1). I first met Weetaluktuk in 1977 when Bob McGhee introduced me to him at what was then the National Museum of Man in Ottawa. Over the course of several years (1977–1982), Weetaluktuk and I maintained regular contact: engaging in correspondence, trading information, sitting on committees, attending conferences, and exchanging visits up north and down south (Martijn n.d.). Across the international border, hospitality was also extended to Weetaluktuk by professionals such as Elmer Harp, Jr., and Allen P. McCartney who took a personal interest in him. Indeed, his initial field training was with McCartney on Somerset Island in 1976, followed by a training session at the University of Arkansas in early 1977 and a subsequent summer expedition to Devon Island with McGhee. During the summer of 1979, in a switch from the usual state of affairs, Weetaluktuk actually hired a southern graduate student, Jean-Luc Pilon, to serve as his field assistant.

Bill Kemp (1982) and Allen McCartney (1984) have pointed out that Daniel Weetaluktuk was an authentic phenomenon in arctic field archaeology. As his interests expanded, he found a direction for himself and became completely engrossed in it. Highly motivated and an indefatigable worker, keenly observant, inventive and eager to try out new surveying techniques adapted to arctic conditions, rapid in absorbing scientific information from academic acquaintances and integrating it with traditional knowledge from Inuit elders to gain new archaeological insights, he acquired increasing competence in administrative matters and in the planning and execution of survey and salvage excavation projects. A list of Weetaluktuk's manuscript reports is appended to the end of this chapter. In the fall of 1979, Weetaluktuk's field notes relating to his survey of the Sleeper and Hopewell Islands in Eastern Hudson Bay, and his salvage excavation project in Inukjuak, were lost when his baggage was stolen at the Great Whale River airport. It was a devastating blow from which he took a long time to recover. Relying on his memory, he wrote up what he could recall (Weetaluktuk 1979c:1; 1980c:1; cited below p. 212).

He was also deeply concerned about the role of native communities in arctic archaeological research, proposing measures for the formation of Inuit personnel and for their increased participation in every phase of this discipline, including decision making. In this connection, his services as a resource person were progressively extended to other communities. Weetaluktuk's all too brief career ended on August 4, 1982: he died while piloting an ultralight airplane, which crashed into the Nastapoka River when the motor stalled.

Shortly before his death, Daniel Weetaluktuk stated to a journalist that "you need commitment and patience" in carrying out archaeological work (Anonymous 1982). He was himself the very embodiment of this declaration. At a later date, the school in his home village of Inukjuak was named after him, and the Ministère des Affaires culturelles du Québec participated in the creation of a "Weetaluktuk Student Prize," which

is handed out annually by the Canadian Archaeological Association. In 1993, the Ministry also contributed to the construction of the Daniel Weetaluktuk Museum in Inukjuak, which is directed by a board of seven elders (Ohaituk 1994:8). The years from 1977 to 1982 are engraved in my mind and, at least to me, will always constitute the Weetaluktuk Period of archaeological history in Nunavik.

Apparently overlooking Weetaluktuk's contributions, Plumet speaks of a "sudden interest in archaeology expressed by the Inuit" emerging in the early 1980s. According to Plumet, this Inuit Period began its existence when his own Tuvaaluk program called a pause to fieldwork in order to begin concentrating on the analysis of data collected during the seventies. This pause, he adds, permitted "the Inuit and the Quebec government to establish a new deal." By this he appears to mean the creation of the Avataq Cultural Institute, which began operation on November 1, 1980. Its archaeology division, however, did not come into existence until June 1985 (Martijn 1994). Although those in the profession tend to equate Avataq exclusively with archaeological work, this institute actually has a far broader mandate, namely that of looking after the cultural interests and needs of the fourteen Inuit communities in Nunavik. These include Inuit toponymy, Inuit language development and preservation, Inuit family genealogy and surnames, Inuit traditional medicine, the promotion of Inuit traditional skills, and, of course, Inuit archaeology and museology. Inuit Elders are closely involved in these programs. The provincial government funding for Avataq's archaeological department, initially under the direction of Ian Badgley and then of Daniel Gendron, primarily covers staff salaries to ensure administrative and scientific continuity.

Three basic principles underlying the provincial government's financial support for such native cultural institutions were set out in a Quebec cabinet policy statement on February 9, 1983:

[1] Quebec recognizes that the aboriginal

peoples of Quebec constitute distinct nations, entitled to their own culture, language, traditions and customs, as well as having the right to determine, *by themselves*, the development of their own identity.

[2] The aboriginal nations have the right to have and control, within the framework of agreements between them and the government, such institutions as may correspond to their needs in matters of culture, education, language, health and social services as well as economic development.

[3] The aboriginal nations are entitled, within the framework of laws of general application and of agreements between them and the government, to benefit from public funds to encourage the pursuit of objectives they esteem to be fundamental (Quebec Government, 1990:7).

For additional details, see the document issued by the Secrétariat aux affaires autochtones (Quebec Government 1988), as well as recent proposals regarding Quebec native heritage conservation contained in the Arpin Commission report (Arpin 2000).

Plumet seems to be ill-informed about these comprehensive cultural policy decisions judging from his belief that "the Inuit as well as the Quebec and Canadian governments probably felt it was easier to negotiate the symbolic value of the archaeological heritage than the economic problems of the . . . rich mineral resources." Apparently unable to envision archaeology from other than an academic research perspective, he ignores the fact that the management of archaeological resources across a vast territory such as Nunavik calls for measures, educational as well as administrative, that do not necessarily concur with his own outlook. Hence, his belief that, "the New Quebec section of the Ministry of Cultural

Affairs and Avataq began a new archaeological project without any consultation with the university."

Questions need to be raised regarding the manner in which archaeology has been practiced and controlled up north by outside researchers and institutions. What kind of feedback is being received by the native population about their own heritage? Community members must be taught the value of this heritage, for such an appreciation is not innate. They should be taking part in decision making processes relating to the preservation and usufruct of those cultural resources. It

would be useful in this connection to cite certain observations by Daniel Weetaluktuk. In his experience (Kemp 1982:11-12):

Many Inuit do not know how useful archaeology is because no one ever bothered explaining its significance to them. All they ever see is some archaeologists that come during summertime then leave again before the snow comes so the Inuit has never become too interested. . . . Arctic archaeology has always been the southern archaeologists' thing for over the past 50 years and still is today. They have kept it that way so the Inuit of the eastern Arctic still doesn't know and understand it too well. . . . So the Inuit has

had to settle for being guides and they had little choice but to do so because of circumstances involved, their lack of inside knowledge and proper training. . . . If the regular research cannot accommodate Inuit needs, then there should be parallel research designs for this purpose.

In 1983, echoing Daniel Weetaluktuk's vision, Inuit elders and community leaders expressed a wish to Avataq that more effort be made to include young



16.1/ Daniel Weetaluktuk at Patterson Island

people in archaeological programs (Avataq Cultural Institute 1985:245). The formula of a field school was eventually decided upon as a practical approach and has been applied during the course of several summers. Thus far, more than seventy students representing practically every Nunavik community have benefited from this experience. As a result, an increased understanding of the aims and techniques used by prehistorians has been disseminated at local levels. Such projects are run by Avataq staff members and qualified graduate student assistants in compliance with government archaeological permit regulations requiring a specific supervisor/student ratio.

Plumet evidently does not consider this as a valid example of what he terms "new collaborative efforts between academic scholars and students and the Inuit interested in their arctic heritage." In his eyes, "the Tuvaaluk Project offered an excellent training program" and he feels aggrieved that "quite different choices were made." All these affirmations contradict what he relates elsewhere, namely that the senior staff of his research team had decided pause fieldwork in order to begin analyzing data from the preceding decade. It is not clear how this situation would have meshed with Avataq's emphasis on field schools and its implications for contract archaeology. What is more, there appears to have been an exodus from his Laboratoire d'archéologie during that time since "several students left for contract archaeology or to continue graduate courses in other universities, since the University of Quebec in Montreal did not offer a graduate program in archaeology." Furthermore, he writes, "working as a research assistant in an academic program was no longer attractive, except to the few who were deeply scientifically minded and were ready to accept long-term training and low incomes." In passing, it should be noted that several former student supervisors at Avataq projects have gone on to graduate studies, three M.A. theses using Avataq data have been completed, and an additional M.A. thesis and one doctoral dissertation are in progress. During the late 1990s,

despite unprecedented objections (Plumet 1996), Avataq associated itself with specialists from Laval University in Quebec to carry out a multidisciplinary study of several Dorset Eskimo petroglyph sites in the region of Kangirsujuaq along the south shore of Hudson Strait (Arsenault et al. 1998; Gendron et al. 1996). More recently, Avataq has joined forces with researchers from Laval University and McGill University to undertake a long-term investigation of the prehistoric and historic archaeological heritage of Nunavik, under the auspices of the ARUC/CURA program (Community University Research Alliances) of the Social Sciences Research Council of Canada.

Since no reference to them is found in Plumet's disquisition on his Inuit Period, I would like to dwell briefly on a few other points. With a grant from the Quebec Ministry of Culture, a booklet explaining Indian and Inuit archaeology, aimed in particular at native communities, was published by the Musée du Bas-Saint-Laurent (1987). It is amply illustrated with photographs showing native students engaged in various kinds of archaeological activities. It was felt that this might help to change the image of archaeology as an exclusive *qallunaat* preserve. In past years, a number of young Inuit have participated in laboratory activities at the Avataq office in Montreal. This same institute also publishes a cultural magazine for the Nunavik Inuit called *Tumivut* whose readership to date has been provided with eight popularized articles on arctic prehistory topics. Finally, a number of archaeological exhibits have been held in northern communities.

Avataq also plays a part in the overall management of archaeological resources within Nunavik. During the past two decades, government laws requiring environmental impact studies and mitigation operations prior to the start of development and construction projects have been instrumental in preventing the wholesale destruction of archaeological remains throughout the Province of Quebec. This legislation stimulated the development of contract archaeology, something which Plumet, perhaps as a purist, regards

with a certain ambivalence: "... it is contract archaeology that has dominated northern Quebec archaeology during the Inuit period. From 1984 to 1992, these projects drained at least \$1.6 million from the Ministry of Transport and the Ministry of Cultural Affairs." Elsewhere he confirms, however, that his Tuvaaluk program alone had been endowed with nearly \$1 million.

According to Plumet, "From 1980 to the end of the 1990s, so little information has been published that it is difficult to follow the development of archaeology in Arctic Quebec." In a recent review of Nagy (2000b), the first monograph to appear in Avataq's new *Nunavik Archaeology Monograph Series*, he writes that "the doctoral thesis of Murielle Nagy is the first archaeological publication by the Avataq Cultural Institute, active in this discipline for about 25 years" (Plumet 2000:157 [author's translation]). This statement, unfortunately, is both erroneous as well as misleading. First, the Avataq Cultural Institute was founded in 1980 (twenty years ago), but its archaeology division was not created until 1985 (fifteen years ago). Second, while Nagy's thesis is in fact the first "monograph" in Avataq's new publication series, the Institute's archaeological staff thus far has published fourteen articles in bona fide scientific journals, with an additional four in press. They have also contributed eight popular articles (trilingual) to the Inuit cultural journal *Tumivut* and presented thirty-five papers at scientific meetings and colloquia. In addition, three M.A. theses and forty-five field reports are available for consultation at Avataq's Montreal office, and copies of almost all of these have been deposited with the documentation center at the Ministère de la Culture et des Communications in Quebec City. As for the artifact collections, some of these can be examined in Montreal, while others are stored at the Conservation Center in Quebec. Granted, this does not constitute an ideal situation, but it is not an uncommon one either in archaeological circles where information is passed on at more than just one level. With a bit of determination and time, any researcher interested in Nunavik prehistory can have access to

Avataq's archaeological data. Even in the best of all possible worlds, we often have to grapple with different realities and be inventive with solutions. Plumet is certainly right in pressing for more frequent monograph production by Avataq, and he is to be commended for the regular appearance of his own archaeology reports in the *Paléo-Québec* series over the past two decades. Hopefully, he will continue to persevere in the task of analyzing, synthesizing, and publishing the extensive remaining data from his Tuvaaluk program.

Plumet relates that, "Yves Labrèche, an early assistant in Tuvaaluk and by this time a Ph.D. student at the University of Montreal, received a small grant from the Quebec Ministry of Cultural Affairs and the Secretary of the Autochthonous Affairs, through the University of Quebec in Montreal, to carry on limited ethnoarchaeological research with the Inuit around Kangiqsujuaq from 1985 to 1989." I would like to add that Labrèche (1989) also received funding from the Ministry to join a multidisciplinary expedition that carried out extensive scientific research at Pingaluit, the famous crater of Nouveau-Québec. Labrèche investigated historic archaeological remains along the crater's rim and around its slopes. Incidentally, he is a colleague who has gained credit for ensuring the participation of Inuit in every one of his field projects.

In a dismissive footnote, Plumet uses terms such as "settling of scores," "manipulations," etc., to describe various attempts at creating a Quebec archaeological association. Each one of us carries away his own impressions of such events. As always, when different personalities come together, ambitions clash and conflicts arise. What stands out in my mind about those early days is the exhilaration most of us felt at the prospect of creating a professional discipline and putting Quebec prehistory on the map. By the time the present Association des Archéologues du Québec (A.A.Q.) came into existence, a new generation of archaeologists had appeared on the scene. Those of the older generation, who still dreamed of exercising leadership roles, found that times had changed. A *modus*

vivendi exists today, with non-members regularly attending annual meetings where everyone can benefit from professional papers and discussions. As proof of this vitality, one needs only to take a look at publication series such as *Archéologiques*, which makes available texts delivered at conferences; *Recherches Archéologiques au Québec*, which provides in capsule form details about the projects carried out each year across the province; and *Mémoires Vives*, which is devoted to historical archaeology.

In his closing remarks, Plumet expounds on the reasons why, supposedly, there happens to be so little Quebecois interest in arctic archaeology. He hypothesizes that, "in Arctic Quebec the cultural and archaeological heritage is directly linked to the Inuit population. . . . This heritage is largely alien to French Quebec culture, but its rapid, successful, and peaceful takeover by the Inuit, as well as the increasing trend toward self-rule by Inuit throughout the North American Arctic, is in contrast, at least in a symbolic sense, with the slow, conflict-ridden, and halting progress of Quebec society toward its own autonomy." Furthermore he states that, "in southern Quebec as in southern Canada, the relationship with the Amerindians and their heritage is different. Amerindian communities had a history that was much more deeply linked with that of Quebec." Quoting statistics, he adds that an increasing number of Quebecois now claim Amerindian ancestry, and ends up by affirming that, "today, Quebec archaeology is much more oriented toward Amerindian archaeology, and especially toward contact and historic period archaeology."

More mundane explanations might be the dearth of full-time job opportunities in Quebec arctic archaeology, or the costs involved in mounting expeditions up north compared with similar activities in the south. As indicated earlier, another factor is the ever-growing menace of site destruction within densely inhabited southern areas that require large-scale and sustained interventions. It is not without reason that the Quebec Ministry of Culture and Communications has specific

funding agreements with Quebec City and Montreal to safeguard archaeological resources, prehistoric as well as historic, in an urban renewal context. As to whether a remote biological affinity explains what Plumet interprets as a special attachment of Quebecois to Amerindian prehistory is a debatable point and, I suspect, largely irrelevant. Sites in the south are relatively more accessible, often more endangered, and, comparatively speaking, present a range of research problems just as intriguing as sites up north. *Taima*.

Acknowledgments

I wish to thank William Fitzhugh and Stephen Loring for their comments on a previous draft of this paper, and I am obliged to Claude Pinard and Daniel Gendron for information on past and present archaeological activities engaged in by the Avataq Cultural Institute.

Note

1. As an aside, the first recorded description of Inuit stone structures in Hudson Strait, on one of the Digges Islands, appears to have been made in 1610 by Abacuk Prickett, a crew member of Henry Hudson. According to his account, "Passing along wee saw some round hills of stone, like to grass cockes, which at first I took to be the worke of some Christian. Wee passed them by, till we came to the south side of the hill; we went unto them and there found more; and being nigh them I turned the uppermost stone, and found them hollow within and full of fowles hanging by their neckes" (Asher, 1960).

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Reply to Charles Martijn's "Second Opinion"

BY PATRICK PLUMET

There is no history without 'histories'—and without witness and historical bias as well. Such histories, however, help incline 'history' towards objectivity. My original presentation clearly states my bias as an *actor in* rather than an *observer of* archaeological research in Nunavik. I therefore welcome Martijn's "second opinion" as it helps place the development of Nunavik archaeology into the wider field of Quebec archaeology, a subject about which Martijn has already devoted considerable attention in previous papers. But some clarification is needed.

My view of this history emphasized my direct experience. Martijn correctly identifies two points (among many others) that I did not discuss: a preliminary archaeological survey he conducted in 1988 with Yves Labrèche in the vicinity of the Nouveau-Québec crater and the important role Daniel Weetaluktuk played in the development of the Inuit role in Quebec archaeology. Concerning the latter, it was, I think, in the fall of 1978 that Robert McGhee phoned me to suggest that the Tuvaaluk Project should consider hiring Weetaluktuk for the 1979 field program, recommending him strongly as a promising young Inuit archaeologist and noting that a special grant might be available for this purpose. I welcomed this opportunity but was later informed that Weetaluktuk had accepted another position. Later I met Daniel two or three times and had a chance to show him the Tuvaaluk collections. He struck me as a perspicacious but shy fellow who might not feel comfortable as part of our French-speaking team. I was never able to form a professional opinion about his work because most of Daniel's archaeological activity in Nunavik postdated Tuvaaluk, and most of his written contributions cited by Martijn were unpublished reports on file at Makivik Corporation or at the Ministry of Cultural Affairs and were held in confidential status for five years after their deposit.

Concerning the issue of Inuit participation in field programs, it is important to remember that the Tuvaaluk

project was organized in less than three months in the fall of 1974. At that time, there was no sign of local interest in our research program from the Inuit. During the summer of 1974, an Inuit couple from Quaqtaq and their children joined my family, which included two young boys, to share three weeks of common experience, mainly in Akpatok Island where an archaeological survey had been planned, and in a hunting camp in Airartuuq, a small island of northwestern Ungava Bay, and also on Diana Island. This was hardly an academic experience, as the project was partly sponsored by Explo Mundo, a documentary film company from Montreal, which filmed the interaction between the two families as we went about our daily life conducting archaeology and pursuing hunting activities. A popular book for children, in both French and English editions, resulted from this experience, which, among other topics, noted an interest in Inuit heritage.¹ Copies of this book were provided to the villages of Nunavik. But even in 1975 and 1976, when the Tuvaaluk field program began, Quaqtaq Inuit continued to show little interest in archaeology. Had the project been conceived in 1977 or later, I imagine the Inuit community might then have taken an important role in the project's development. In that case, rather than being the last project in my "Quebec period," it would have been the first of the "Inuit period," for, as mentioned in my paper, the political situation in northern Quebec and the attitude of the Inuit toward archaeological research changed rapidly after 1977.

As an archaeologist, I did not feel comfortable engaging in political activism, but with regard to the 'might have beens' of Martijn's criticism of the Tuvaaluk Project as overly academic in orientation, I certainly would have been enthusiastic about a broader community approach. On several occasions, beginning in 1981, I suggested to Martijn and/or to Michel Noël, who were then in charge of New Quebec archaeology at the Ministry of Cultural Affairs, that the Tuvaaluk Project might play a role in the development of Inuit archaeology by conducting a field school. During this

period I also explored with my university administration the possibility of obtaining space for laboratory work by Inuit students who had by then begun to express an interest in archaeology. It is unfortunate that none of these suggestions took root, since Inuit students at that time likely would have been interested in a formal program of archaeological study. As it happened, such programs did not begin until the late nineties, as mentioned by Martijn, when Daniel Arsenault, a scholar from the CELAT center at Laval University, became involved in Avataq research.

I never felt “aggrieved,” but I was certainly puzzled by the way new archaeological research was begun again in New Quebec under the instigation of the Ministry. When an archaeologist contemplates starting a project in a site or an area which has recently been studied by another archaeologist or professional team, professional courtesy as well as scientific efficiency calls for consultation with others who have conducted work there previously, especially as they may consider returning. William Fitzhugh consulted with me before starting his program in northern Labrador, and we both benefited from the ensuing exchange of information. I had a similar exchange with Elmer Harp following 1972 and with an archaeologist working in Blanc Sablon when I became involved in 1989 with research at Paleo-eskimo sites in that region. However, I learned of new work in Nunavik only by rumor, months after it had begun, in communication with Ian Badgley. This research was directed at the important Groswater site that had been discovered near Quaataq during the last year of the Tuvaaluk Project, and at Nunainnguq in western (McLelan) Strait, where preliminary field work had been carried out by the Torngat Archaeological Project and the University of Quebec in Montreal team in 1978 and 1979.

Martijn is misinformed when he states that I never hired a single Inuk for my excavation crew. In the mid-seventies, three young Inuit selected by the Quaataq community were hired for excavations at Tuvaaluk site. They were welcomed by all, participated

in the scientific work, and had to eat our mostly dehydrated food. As it turned out they did not enjoy the meticulous excavating procedures, disliked the sedentary nature of digging, and expressed dissatisfaction with our food, and after a week departed and returned to their village.

I also assisted the emergence of a First Nations archaeology program beginning in the early seventies, following a meeting with William Craig which was organized by Bruce Trigger at McGill University in March 1970. As the need for such a program had been expressed by the Intertribal Council of Native Students, I obtained an agreement in principle from my newly-founded university (1969) to support cooperative programs in Native North American studies for Native students. The project evolved first within Loyola College and McGill University (Craig 1972) before shifting for a brief time to Manitou College of La Macaza, north of Montreal, which finally closed around 1976 (Beaudoin 1977).

I still argue, as I have recently in the case of Kennewick Man (Plumet 2000), against the ownership of archaeological remains, not particularly by the First Nations minorities as has been insinuated by Martijn, but by any individual, collectivity, or any administrative or political entity such as a municipality, a state, or a nation, although such entities should be responsible for the preservation of archaeological sites as part of world heritage. I also do not believe that archaeology can or should be used to establish territorial or political rights. But I cannot let Martijn’s other unfounded insinuation stand, that my reasons for these beliefs are based on a fear that such ownership infringes upon my right of access to archaeological research! My reasons for these beliefs have been clearly stated in several papers and have nothing to do with personal motives.

The development of archaeology in Northern Quebec is not just academic history. As a Neo-Quebecker—a newcomer—in Canadian and Quebec society, I directed my work toward specific goals: the prehistoric archaeology of Arctic Quebec and the development

of my discipline, as it had been requested by my university. I did not have the personal legacy of earlier generations to motivate me to take up the political causes that Canadian, Quebecers, or the Inuit sometimes engage in. When I expressed my views publicly, I tried to do so as a witness and as one who was explicitly a neo-Canadian or neo-Quebecker, avoiding any personal identification with any cause. But as an actor in a profession that was linked to ideological and political interests, I have also been a pawn, and I have both benefited and suffered from the circumstances. As such, I did not express publicly my personal feeling about these circumstances and did not allow myself to be personally affronted when they were unfavorable. But I tried, often unsuccessfully, to understand the process in which these circumstances occurred. I still do not feel able nor authorized to carry on any systematic historical investigation on these subjects.

But to conclude, I would like to suggest some research avenues for future historians. It might be instructive and enlightening to investigate more thoroughly the role of factors such as the complex and very sensitive intercultural relationships and the cultural, linguistic, and political rivalry which—from the sixties on in Quebec—have been especially exacerbated

and complicated by being frequently disguised in a vague and very “politically correct” manner. Such sensitive interactions have not been limited to any one population, but only the prehistoric and historic founding populations could realize their quest for self-image through archaeology. The roots of what might be perceived as a “blinkered” anti-academic bias at the beginning of the Avataq archaeological program remain to be understood in its sociological and cultural context, and, possibly, its consequences need to be appraised. The results of the Tuvaaluk project should also be judged not only in terms of its contribution to archaeological knowledge, but also on its impact on the development of Inuit archaeology as represented through its legacy in Avataq, which borrowed many Tuvaaluk methods and terminology. And even more broadly, one might consider whether conducting archaeology in French, as has been done in Quebec Arctic since 1967, was a blind ally in terms of the evolution of Nunavik archaeology and the emergence of its Inuit identity.

Note

1. *Poutoulik chez les Inouit*, and *Putulik with the Inuit*. Montreal, Heritage, 1977 and 1978. Text by Nicole Rich-Plumet, photographs by Patrick Plumet.

Early Bird Archaeologists among the Bake Apples: A Quick Swoop along Quebec's Lower North Shore

JEAN-YVES PINTAL (PICTURED)
CHARLES A. MARTIJN

The Lower North Shore of Quebec forms the north-eastern coastal boundary of the Gulf of St. Lawrence and stretches for more than 400 kilometers from the village of Kegaska, opposite the eastern tip of Anticosti Island, to Blanc Sablon, at the western end of the Strait of Belle Isle (fig. 17.1). Inside the Gulf, myriad islands and archipelagos are strung along most of its heavily indented coastline. A cold continental climate prevails, with long severe winters but relatively little snowfall, and short wet summers marked by frequent fog. The rich and varied marine fauna has served as a mainstay for human occupation for at least 8,000 years.

The Early Elders

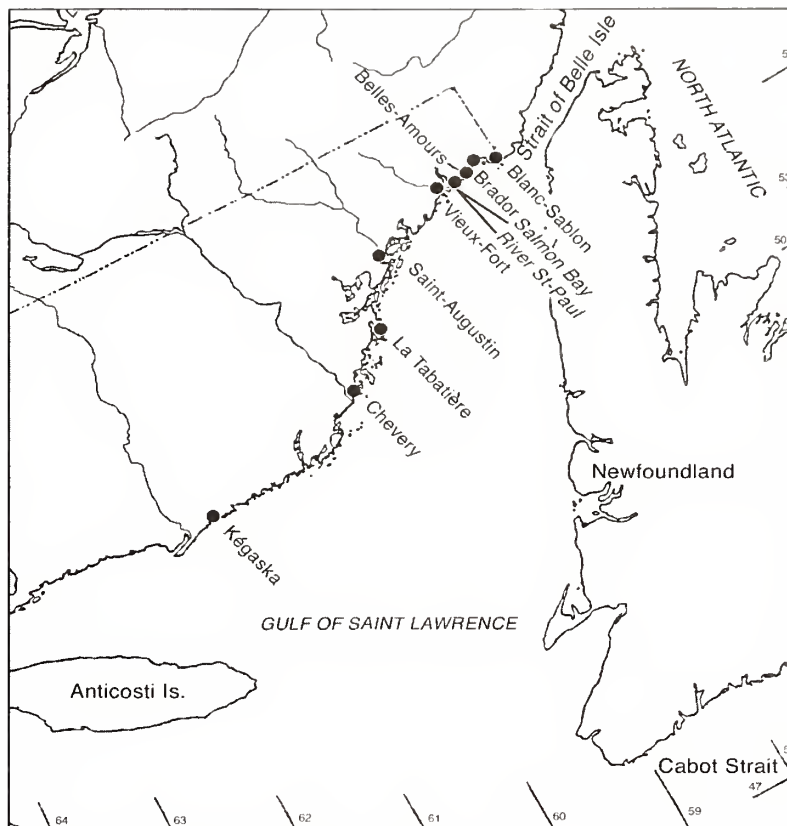
More than a century ago, T. G. B. Lloyd (1874) described a collection of prehistoric Amerindian objects found on the coast of southern Labrador and, among other things, speculated that some of the archaeological remains that occurred in Newfoundland might turn out to be "mountaineer" (i.e., Innu) sites of the historic period. Martijn (1990a) subsequently compiled an ethnohistorical record of Innu/Montagnais hunting and trapping voyages to western Newfoundland. The possibility that such small seasonal population movements across the Strait of Belle Isle represented a traditional pattern practiced by the prehistoric ancestors of the Innu/Montagnais now has been confirmed at the North Cove site (EgBf-08) near Ferolle, Newfoundland (Hull 1999:16, 19). It has also been proposed that the Beothuk were traveling to the north shore of the



Strait of Belle Isle, mostly in the Blanc Sablon area (Marshall 1996; Pintal 1998).

The first bona fide archaeological work in the area of the Quebec Lower North Shore appears to have been done by Alfred V. Kidder who, while on his way to Labrador in 1910, made a stopover at Blanc Sablon. The results of his reconnaissance, however, were never published (Fitzhugh 1972b:1).¹

During the late 1920s, William J. Wintemberg carried out a more intensive survey project at selected locations along the entire length of the Quebec North Shore. Many of us tend to identify Wintemberg with Iroquoian studies and think of him primarily as an Ontario archaeologist. During the 1920s and 1930s, however, he was quite active in other parts of Eastern Canada as well (Jenness 1941; Swayze 1960). In fact, as far as Quebec is concerned, Wintemberg was the first person to provide that province with an idea of the geographical scope and cultural variety of its archaeological resources (Martijn 1979:8). Many well-known sites in Quebec, such as Lanoraie, Batiscan,



17.1/ Map of Quebec's Lower North Shore

Mingan, Kegashka, and Brador, were all initially investigated by Wintenberg. His research interests, however, covered a much more extensive area. In fact, it can be said without exaggeration that his footprints can be found everywhere around the rim of the Gulf of St. Lawrence, from the North Shore to Newfoundland, Nova Scotia, New Brunswick, and Prince Edward Island. Several decades later, when Richard Pearson and Charles A. Martijn carried out preliminary work on the Magdalen Islands in the middle of the Gulf, they started off by retracing Wintenberg's footsteps there as well (McCaffrey 1986:111–113).

Someone should take on the task of gathering together and annotating Wintenberg's personal and scientific papers in order to provide us with a better perspective on what specific research interests initially drew him to the Gulf of St. Lawrence region. Considering his general state of health, it is truly amazing that he covered such a vast territory. Born in 1876 in New Dundee, Ontario, he was frail at birth; doctors

diagnosed him with a weak heart. He started school at a late date and was obliged to stop during his early teens. A job as a compositor with a printing firm in Toronto caused his health to deteriorate even further when lead poisoning affected his lungs. Despite such setbacks, Wintenberg continued to pursue his interests with unflagging energy. He read voraciously, developing a passionate interest in all kinds of subjects, including archaeology. Eventually this led him to seek employment with the Ontario Provincial Museum and, later on, with the National Museum of Man in Ottawa (fig. 17.2). In 1928, at the relatively advanced age of fifty-two, he began a brief career as a subarctic archaeologist, traveling by boat along the

entire Lower North Shore. His trip to Newfoundland the following year, in 1929, unfortunately undermined his physical health and culminated in a series of heart attacks. Thereafter, he restricted his archaeological activities to Ontario until his death, on April 25, 1941, at the age of sixty-five.

It appears that Wintenberg left few records, such as field notes, diaries, or office correspondence, relating to his 1928 excursion along the Quebec North Shore. For details, we have to rely on artifact catalogs and a series of typewritten extracts, which seem to have served as a daily journal (Wintenberg 1928). Wintenberg appears to have engaged primarily in surface collecting and some test-pitting. He recovered several hundred archaeological specimens that can be assigned to a variety of Amerindian, Paleoeskimo and Historic Inuit occupations. He focused his research on two places: Kegaska and the Brador-Blanc Sablon regions. Summary accounts of his North Shore findings were filed with the Canadian Museum of Civilization

(Smith 1929:333; Wintemberg 1928); they were also incorporated into articles synthesizing available information on specific topics, such as the use of shell beads among the Beothuk, possible Beothuk graves in the Brador-Blanc Sablon area, and the geographical distribution of aboriginal pottery in Canada (Wintemberg 1936, 1942). The publications on his investigations at Dorset Paleoeskimo sites in northwestern Newfoundland (Wintemberg 1939, 1940), however, are more extensive.

On the basis of typological similarities, Wintemberg demonstrated that his Newfoundland sites were affiliated with the Dorset culture, which had originally been defined by Diamond Jenness from remains uncovered in Hudson Bay. Wintemberg's hypothesis that most of the Newfoundland coastal zone was once inhabited by Dorset people has been confirmed by more recent research. At the time, opinions varied on whether arctic-adapted Paleoeskimo cultures had evolved out of boreal interior populations. Based on his pioneering excavations in Labrador, William Strong (1930) had initially defined an Old Stone culture that he regarded as a basic Amerindian stratum from which later Eskimo and Indian cultures evolved. Wintemberg, however, underscored the need to develop a more refined cultural-chronological sequence in order to resolve a variety of related questions, for example: Could the Eskimo-like artifacts from Strong's sites be attributed to the Dorset culture? Was it the Eskimo or the Beothuk that had used gouges, stone adzes, long polished knives or lance points, and grooved stone plummets, or were these objects the remains of an earlier Amerindian people? Two questions arising from Wintemberg's work at Kegaska still remain unresolved. First, did he actually obtain Paleoeskimo material there and, if he did, does it belong to the Groswater or the later Dorset culture (de Laguna 1946:108, fig.8; Taylor 1964b:196)? Second, is his Kegaska pottery of Iroquoian or Algonquian origin (Martijn 1990b:51–52; Taylor 1964b:191)? Subsequent surveys at Kegaska



17.2/ William J. Wintemberg at the National Museum of Man

have failed to uncover any trace of Paleoeskimo remains or Iroquoian pottery (Chapdelaine and Chalifoux 1994), but more recent excavations at La Romaine, 75 kilometers east of Kegaska, have led to the discovery of two sites containing Amerindian ceramics from the Middle Woodland period (Pintal 1995, 1996). This discovery suggests that the Algonquians who frequented the Lower North Shore obtained their pottery through exchanges with trading partners from areas located along the upper St. Lawrence valley where Amerindian ceramics are abundant. The scarcity of Late Woodland ceramics in the area suggests that the St. Lawrence Iroquoians visited the Lower North Shore infrequently during this period, but this pattern changed after they came into contact with Europeans, who encouraged them to travel further from their homeland (Martijn 1990b).

In his methodical way, Wintemberg steeped himself in the available literature dealing with arctic prehistory and incorporated this information in the concluding sections of articles relating to his North Shore and Newfoundland excursions. Clearly respected by his North American colleagues, he was invited to review William Duncan Strong's *A Stone Culture from Northern Labrador and its Relation to the Eskimo-like Cultures of the Northeast* (Wintemberg 1930). His publications and judicious views were cited by Frederica de Laguna

(1946) in her well-known synthesis article "The Importance of the Eskimo in Northeastern Archaeology," and she paid tribute to him as "our beloved and regretted Wintemberg" (de Laguna 1946:10). His obituary was written by Diamond Jenness (1941). Another Ottawa colleague, Douglas Leechman, in collaboration with James B. Griffin, submitted a manuscript, which was found among Wintemberg's effects at the time of his death, to *American Antiquity* for posthumous publication (Wintemberg 1942).

Although he was restricted by circumstances from making a major contribution to northern prehistoric archaeology, Wintemberg, nevertheless, won recognition from his peers as a full-fledged participant in its developmental stage. Meticulous, disciplined, observant, and endowed with an incisive and questioning mind, Wintemberg must be considered as a sterling example of the arctic archaeology pioneer. Jenness (1941:66) wrote that "without question he was the leading authority on Canadian archaeology, and his advice was sought by scholars everywhere, but he remained always the simple and modest student, eager to learn and to help others in their quest."

Esteemed Younger Elder

Another noted elder, Elmer Harp, Jr., began his prehistoric research program in the Strait of Belle Isle twenty years after Wintemberg's 1928 excursion, but he did not actually visit the Quebec Lower North Shore until 1961 when he worked at Blanc Sablon (Harp 1951, 1964a, 1964b). By this time, archaeological research in the arctic and subarctic, in terms of both field methods and theory, had been transformed. Surveys and excavations were being carried out in a more systematic and detailed fashion. Harp was the first archaeologist to apply these approaches in the Strait of Belle Isle and on the Lower North Shore. He was also the first scholar to propose a chronological sequence for the sites he discovered there, combining an absolute chronology based on radiocarbon dates with a relative chronology tied to raised beach terraces (Table 17.1).

Harp (1964a, 1964b, 1969–1970, 1976a, 1976b) also played a role in refining our thinking about Amerindian cultural chronology, Indian-Paleoeskimo contact in the Northeast, and Dorset Eskimo affinities. Douglas S. Byers (1959) had initially proposed the concept of a Boreal Archaic and subsequently put forward the idea of a distinct cultural phase, the Maritime Boreal Archaic, for the Atlantic coast.² Harp (1964b) integrated his Amerindian data from Blanc Sablon and the Strait of Belle Isle into Byers' scheme and added early, middle, and late subdivisions that were distinguished on the basis of projectile point and knife types, the variability of lithic raw material, and the absolute altitude of the terraces on which the sites were located. Harp and Hughes (1968) eventually formulated a more elaborate chronological sequence for the entire Strait of Belle Isle region, and the publication of their radiocarbon dates, some of which are very old, have attracted the attention of archaeologists interested in the Archaic period of the Northeast. They also demonstrated that some Amerindian occupations preceded the arrival of the Paleoeskimo and found that evidence for contact between these two groups was much more difficult to establish than had previously been supposed.

A lucid writer, an able teacher, and a gentleman of the old school, Harp served as an inspiration to the succeeding generation of researchers in this area, several of them his own students. They used his publications as a point of departure in expanding their own ideas and interpretations about arctic prehistory.

In 1988, the Quebec Ministry of Culture officially classified the western section of the Blanc Sablon River mouth as a protected archaeological zone. This decision was made largely on the basis of Harp's 1961 pioneering work, which drew attention to the exceptional quality and variety of the prehistoric remains concentrated in that sector (Pintal 1998).

Contemporary Practitioners

Among more recent practitioners is J. A. Tuck whose excavations at the Port-au-Choix cemetery in

Newfoundland redefined the Archaic Maritime tradition for the northeast (Tuck 1971). Tuck showed that this period was characterized by a continuous adaptation to a given environmental setting by a single population practicing a similar way of life. In subsequent work along the north shore of the Strait of Belle Isle, McGhee and Tuck (1975) formulated a detailed regional chronology of human occupation extending from 9000 to 3000 B.P., which was marked by a fluorescence between 7500 and 3500 B.P. On the basis of lithic analysis, they concluded that there had been no drastic cultural transformation but only gradual change through time involving a number of variables. Later research activities extended this occupation period within the Strait to 2000 B.P. (Madden 1976; Tuck 1982).

Although William W. Fitzhugh (1981) made only one brief visit to the Lower North Shore at Île-au-Bois before initiating research in the area in 2001, his large-scale investigations in Central Labrador allowed him to identify a series of archaeological complexes and phases (Paleoeskimo as well as Amerindian), propose a chronological sequence, and elaborate a set of cultural models and subsistence-settlement systems. While modified over the years, his work still serves as the basic theoretical framework for the entire Far Northeast (Fitzhugh 1972b). Fitzhugh's realization that some Amerindian cultural units formed a continuum spanning several millennia while others were disrupted between phases led him to question the validity of Tuck's hypothesis of a single continuum that incorporated all regional cultural manifestations (Fitzhugh 1975). In addition, his study of subsistence and settlement patterns raised questions about the validity of the concept of a constant, unvarying dependence on maritime food resources. Fitzhugh suggested that this type of exploitation had changed through time, with some Amerindian groups exploiting maritime food resources only intermittently while others relied on both maritime and terrestrial fauna for their daily sustenance. It is obvious

that a balanced perspective on the variability of cultural adaptations in the Far Northeast can only be achieved by organizing research programs oriented toward both interior and coastal sites.

The archaeological surveys carried out by Charles A. Martijn (1974) in the Salmon Bay/Rivière Saint-Paul/Vieux-Fort area demonstrated that this area compared closely with the Blanc Sablon district and Newfoundland and Labrador in terms of the quantity of sites, cultural variety, and chronological sequences present. Martijn initiated an evaluation of the prehistoric role of the Saint-Paul River as an interior link between the Lower North Shore and the Labrador coast; he was also the first person to undertake ethnoarchaeological research in the area. The work done by René Levesque (1976) at Blanc Sablon and Brador, although primarily oriented toward surface collecting, provided a cross-section of the extensive local prehistoric remains tied to a chronological framework.

Until the mid-1980s, archaeological projects in the Strait of Belle Isle itself had uncovered little information relating to late Amerindian prehistory (2000 B.P.–contact). McGhee and Tuck (1975:126) suggested that this lack of data could be attributed to the existence of hostile relations between Amerindian and Paleoeskimo populations between 2500 and 1500 B.P., leading the Amerindians to retire inland. They also postulated that after the Paleoeskimos left, the Amerindian groups in the area failed to readapt to coastal subsistence practices, thus turning the Strait into a kind of “no man's land.” This situation was thought to have lasted until the arrival of European fishermen during the sixteenth century. Following Harp's pioneering efforts, extensive research activities in the Brador/Blanc Sablon area during the past decade, beginning with Levesque's (1976) work, have substantially modified this interpretation. It is now clear that the western extremity of the Strait was frequented by both Inuit and Indian groups on a regular basis during Recent Prehistoric times (Pintal 1989), while Indian groups alone occupied the rest of the Lower North Shore.

An extensive interdisciplinary archaeological program undertaken between 1983 and 1990 in the La Tabatière/Blanc Sablon region under the aegis of the municipality of Blanc Sablon and the Quebec Ministry of Culture has considerably improved our comprehension of local Amerindian and Inuit occupations (Groison et al. 1985; Pintal 1998). Directed by Jean-Yves Pintal, this program emphasized extensive site excavations, settlement pattern analysis, and the study of adaptive systems. A systematic survey in the region led to the identification of nearly 200 sites, twenty of which, thus far, have been excavated (about 1,000 square meters). More than fifty radiocarbon dates are now available, providing a solid chronological framework. Field reports, computerized data on site locations and their cultural content, and related bibliographies can be consulted by contacting Inventaire des sites archéologiques du Québec (ISAQ) at the Ministère de la Culture et des Communications in Quebec. The archaeological collections themselves are stored at the Laboratoire d'archéologie of the Centre de Conservation.

Additional field projects, consisting mainly of salvage operations connected with road construction, sewage systems, and hydroelectric development, have also been carried out at localities further west, such as Belles-Amours, Salmon Bay, Rivière St-Paul, Vieux-Fort, St-Augustine, La Tabatière, Lac Robertson, La Romaine, Musqua and Kegaska. More than sixty field reports and articles that deal with the prehistory of the entire Lower North Shore have accumulated. The synthesis of this data (Pintal 1998) has bolstered the concept of a distinct chronological framework based on the definition of new phases and complexes. It also has provided insights on the intra-site spatial organization through time, and mobility and settlement pattern characteristics for the prehistoric population.

Lower North Shore Cultural-Chronological Sequence

The initial intrusion of Amerindians into the Quebec Lower North Shore region appears to have taken

place more than 8,000 years ago (Table 17.1). McGhee and Tuck (1975) have proposed a cultural continuity between the small triangular points found on the earliest campsites and Paleoindian remains from the Maritime Provinces. As yet, there is no consensus about the route that Amerindian groups may have taken. Chevrier (1996b:86) subscribes to the theory that they came from the Maritime Provinces by way of Cape Breton Island and Newfoundland. Unfortunately, the archaeological record in the Maritimes cannot be examined because most of the coastal prehistoric sites dating to this period (8000 to 5000 B.P.) are submerged underwater because of coastal subsidence (Tuck 1984). On the other hand, Groison (1985:133), in his discussion of the EiBg-7 site at Blanc Sablon, and Lasalle and Chapdelaine (1990:15), in a review of late glacial and Holocene events in the Champlain and Goldthwait Seas areas, have adopted a more cautious stance. They point to the need for additional problem-oriented archaeological research in the St. Lawrence River valley and along the entire North Shore.

The definition of a Maine Archaic Tradition at the beginning of the 1990s (Robinson 1992) has allowed us, to a certain point, to associate the underlying quartz technological tradition with the most ancient sites in the Strait of Belle Isle. It has been suggested that the initial peopling of the North Shore derived from this archaic source and that the region around Quebec City may have served as a departure point for these pioneer populations (Pintal 2000). Similar cultural assemblages have been found in the Quebec area (Laliberté 1992; Pintal 2000) and on the Upper North Shore (Archambault 1994, 1998; Pintal 2001; Plourde 2000).

However, differences in the tool-types discovered along the Lower North Shore and the Upper North Shore now suggest that the initial peopling of these two areas followed two different trajectories. The cultural affinity of the early Lower North Shore hunting groups is basically an Archaic one. These groups practiced a mixed economy based on a foraging type of territorial mobility, and they exploited a wide range of coastal

resources. Their funerary rites, which are associated with burial mounds, are among the oldest known in the Northeast (McGhee 1976a).

From 6500 to 5000 B.P., the region participated in the development of the Maritime Archaic Tradition, and it maintained close connections with this culture in the Strait of Belle Isle, Labrador, and Newfoundland. Sites dating to this period, and to that of the Maritime Archaic climax, have been found between Blanc Sablon and Vieux-Fort (Beaudin et al. 1987; Martijn 1974). A number of other components occur further west, all the way to Kegaska (Pintal 1998). After 5000 B.P., with the florescence of the Maritime Archaic Rattlers Bight complex in Central Labrador, the Blanc Sablon district and the Strait of Belle Isle were relegated to a peripheral position and became cultural backwaters.

If this is the case for the Blanc-Sablon area, the reverse is true for the Mecatina/La Tabatiere region where cultural remains are abundant, some bearing a striking resemblance to artifacts found at the Port-au-Choix cemetery. During this period, it appears that new Amerindian populations already present on the Middle North Shore began to penetrate the eastern sector of the Lower North Shore, specifically, the Old Fort/St. Paul River area.

By 3500 B.P., the Maritime Archaic Tradition had started to decline. The above mentioned newcomers probably assimilated the remnant Maritime Archaic groups, and the Lower North Shore underwent a cultural revival marked by experimentation with different types of environmental adaptations (Pintal 1998). Middle Woodland pottery, as well as Meadowood

Table 17.1/ Chronological sequences proposed for Strait of Belle Isle, Newfoundland, and Central Labrador

B.P.	Strait of Belle-Isle						Newfoundland		Central Labrador	
	Lloyd XIXe S	Harp 1960-1970	Tuck 1980		Pintal 1990		Beothuk	Labrador Inuit	Innu	Labrador Inuit
0	Innu	Beothuk	Beothuk	Labrador Inuit	Beothuk Innu	Labrador Inuit	Beothuk	Labrador Inuit	Innu	Labrador Inuit
1000	?	Late B O R E A L	POST-ARCHAIC	Dorset	Anse Morel Anse Lazy Longue Pointe	Dorset	Little Passage Beaches Cow Head	Dorset	Point Revenge Daniel Rattle	Dorset
2000		Middle		Pre-Dorset Groswater	Blanc-Sablon	Pre-Dorset Groswater		Pre-Dorset Groswater		Pre-Dorset Groswater
3000					Ruisseau Manius				Saunders	
4000		Early		MARITIME ARCHAIC	MARITIME ARCHAIC	Late	MARITIME ARCHAIC		MARITIME ARCHAIC	Late
5000						Middle				Middle
6000						Early				Early
7000										
8000										
9000										

ideological practices and Middlesex ceremonial burial customs, diffused eastward from the St. Lawrence lowlands, most likely through both coastal and interior trade networks (Chevrier 1996b; Loring 1989). In fact, the Lower North Shore has as many, if not more, sites with typical Middle Woodland pottery as it has sites with Upper Woodland ceramics.

From 1000 to ca. 400 B.P., the Lower North Shore was regularly exploited by Recent Prehistoric bands, which practiced a mixed subsistence economy with an emphasis on seal hunting during the spring season. In fact, because of the intensity of seal exploitation from 1500 B.P. onward, these bands may have developed a form of sedentariness in the Blanc Sablon region (Pintal 2000). They maintained strong links with groups on both sides of the Strait of Belle Isle, as well as in the adjoining regions of Southern Quebec-Labrador Peninsula and western Newfoundland (Pastore 1989; Pintal 1989, 1998, 2000; Robbins 1989). In Labrador, the Recent Prehistoric period is represented by the Point Revenge complex, which is distinctive for its use of Ramah chert, and in Newfoundland by the Little Passage complex, which relied primarily on western Newfoundland chert deposits for its lithic raw material. Some scholars suggest that these culture groups were ancestral, respectively, to the historic Innu/Montagnais (Point Revenge for Labrador and l'Anse Morel for the Lower North Shore) and the Beothuk (Little Passage). In the Blanc Sablon district and along the eastern Lower North Shore, the archaeological material from Recent Prehistoric sites is related to remains that characterize the Little Passage complex defined in Newfoundland. Since this material occurs in a different territory, it has been given its own designation, the l'Anse Morel complex (Pintal 1998, 2000).³ But considering the fact that this material has been found all along the littoral of the Lower North Shore, it is likely that it belonged to a local resident group, who alternated occupation sites on either side of the Strait of Belle Isle. Following the European settlement of this area, this group may have merged with groups from the interior of the Quebec-

Labrador peninsula to form the ancestors of the Mamit Innuat (Eastern Innu) population.

With the arrival of European fishermen and whalers in the sixteenth century, the Lower North Shore Amerindians modified their seasonal subsistence cycle. They extended their stays along the coast to the entire summer in order to maximize trade exchanges for the material bounty that these newcomers had to offer.

Paleoeskimo groups (Late Pre-Dorset Groswater) initially appeared on the Lower North Shore around 2800 B.P. To date, nine Paleoeskimo sites have been discovered; these include small camps on Île-Verte and Île-au-Bois, and larger mainland sites at Blanc Sablon, Brador, Middle Bay, and Salmon Bay (Martijn 1974; Pintal 1994; Plumet et al. 1994). Five radiocarbon dates are available for three of these sites. The Paleoeskimo settlement pattern is characterized by the exploitation of coastal faunal resources, primarily during the months of April, May, and June, when large numbers of seal herds frequent the Strait of Belle Isle and its extremities. Comparative studies of artifact assemblages indicate a close relationship with Groswater sites in Newfoundland.

Later Dorset sites (2200–1200 B.P.) are much less common in the Lower North Shore region, especially in contrast to the abundance of local Amerindian remains from the same time period. This may indicate that access to the area by Middle Dorset groups may have been hindered by an increased Amerindian presence. Although the archaeological data provide no specific evidence for contact between Paleoeskimos and Amerindians (Plumet et al. 1994), recent research suggests that the two groups stayed near each other and exchanged certain goods, such as lithic raw material and various types of tools (Pintal 2000; Renouf 1999).

On the basis of ethnohistorical accounts, Martijn (1980) has postulated that, starting in the 1580s, the Historic-period Inuit along the Labrador coast began to make seasonal excursions into the Strait of Belle Isle to obtain European goods at Basque whaling stations.

Furthermore, based on the discovery of various Inuit material objects and a human mandible from one of several stone structures in a boulder field near Rivière Saint-Paul, Martijn and Clermont (1980) have concluded that, between 1640 and 1690, the Inuit gradually extended their winter activities westward into the Gulf. The excavation of two Historic-period Inuit semi-subterranean winter houses at the Bay of Belles-Amours offers additional archaeological support for this thesis (Dumais and Poirier 1994).⁴

During the past few decades, our knowledge of prehistoric events in the Lower North Shore region has broadened considerably. At the same time, the public's understanding of excavation aims and procedures has grown locally and, as a result, the protection of archaeological remains is now better assured. Expanding research objectives and an increasingly sophisticated methodology are adding further depth and ever-larger dimensions to the initial sketches of Lower North Shore prehistory that were drawn by the first elders.

Notes

1. For perspectives on the history of archaeological research in the Lower North Shore region and its cultural-chronological sequence within the wider context of the Quebec-Labrador peninsula, see Chevrier (1996a, 1996b); Cinq-Mars and Martijn (1981); Harp (1984); Harris (1987); Fitzhugh (1972b); Martijn (1988, 1998); Pintal, J.-Y. (1998); Taylor (1964b); Tuck (1975a, 1982, 1984); and Wright (1995).

2. For a general review of the Archaic concept in the Northeast, see Byers (1959); Clermont (1992); Fitzhugh (1972a); Robinson (1992); Starna (1979); Tuck (1975a); and Wright (1972).

3. A review and assessment of the Point Revenge cultural construct presently identified at sites ranging all the way from Labrador to the estuary of the St. Lawrence River would be a worthwhile subject for an archaeological conference session.

4. In another context, Auger (1994) has commented on the relative absence of Historic Inuit remains within the Strait of Belle Isle. There may have been environmental reasons why, during specific periods in the past, native groups did not dally seasonally within the Strait, but used it primarily as a passage way between the Labrador coast and the Gulf of Saint Lawrence.

The Elusive Landry Site in Newport, Gaspé

MOIRA T. MCCAFFREY

Archaeologists working in the Eastern Arctic have long been interested in determining the southern range of Paleoeskimo populations and in documenting evidence for interaction between Paleoeskimo and Amerindian groups (Beauchamp 1899; Fitzhugh 1980b; Martijn and Clermont 1980; Plumet et al. 1994; Speck 1931). As early as 1885, A. S. Packard commented on the nineteenth-century presence of Labrador Inuit as far south as the Quebec North Shore. He suggested that, "the facts we here present should induce our New England and Canadian archaeologists to make the most careful examination of the shell-heaps about the mouth of the St. Lawrence, and on the shores of northern and southern Nova Scotia, as well as Maine and northern Massachusetts for traces of Eskimo occupation" (Packard 1885:473; see fig. 18.1).

During the 1940s, Frederica de Laguna wrote about the possibility of "borrowed traits" in Dorset culture—artifacts that appear "as a result of borrowing from the Indians, or which have been loaned by the Eskimo to the Indians" (de Laguna 1946:111). Both de Laguna and, later, William Ritchie (1951) were particularly interested in the possible derivation of Archaic-period ground-stone technology from Paleoeskimo peoples. Of course, the onset of radiocarbon dating demonstrated that this premise was untenable, since Archaic ground-stone ulus and spear points were found to pre-date Dorset occupation in the Eastern Arctic (Bourque 1995:6–7).

More recently, discussions of cultural interactions between Paleoeskimo and Amerindian peoples have



focused on Dorset and Point Revenge groups on the Labrador coast and the Lower North Shore of Quebec. Particularly intriguing is the evidence for intensive utilization of Ramah chert by Late Prehistoric period groups at a time when the northern coast of Labrador, including the region of the Ramah quarries, was occupied by the Dorset (Loring 1992, this volume; Pintal 1989, 1998).

The Landry Site

As this Chapter demonstrates, an archaeological site purported to have been found during the 1930s in the Gaspé Peninsula of Quebec played a memorable, albeit less than critical, role in the history of research on the southern range of Paleoeskimo occupation. My first encounter with the "Landry site" took place in the winter of 1985 as I was going through boxes of long-ignored archaeological collections in the storage room of the McCord Museum situated in downtown Montreal. I had volunteered to unpack and identify the artifacts, secretly hoping that an important, or at least interesting, collection might be lurking under the old crumbled newspapers.

As I unpacked a small group of what appeared to be Paleoeskimo and Labrador Inuit artifacts, a label fell



18.1/ Map of the the Gulf of Saint Lawrence region and places mentioned in the text.

out of the wrapping paper and caught my attention. It read “Found by Captain Ambrose Landry. Back of Newport, Gaspé, Quebec, in a supposed Eskimo stone igloo.” The artifacts consisted of a tiny ivory bear incised with a skeletal motif, four perforated ivory pendants, an ivory awl, a slate endblade, two miniature steatite lamps, a bone tool fragment, and a small ivory object of undetermined function (fig. 18.2).¹ Entries in the museum’s accession books revealed only that the artifacts had originally been donated to the Redpath Museum of McGill University by Duncan M. Hodgson on September 14, 1939. The objects had come to the McCord Museum during a reorganization of the collections held by McGill University’s museums in the 1950s.

Evidence for Southern Paleoeskimo Sites

The possibility that Paleoeskimo people had visited and camped on the Gaspé peninsula was of particular interest to me in 1985. I had recently begun an

archaeological research project involving the Îles-de-la-Madeleine, a chain of small islands located near the center of the Gulf of St. Lawrence. In preparation for fieldwork, I had examined lithic artifacts surface collected on the islands by William J. Wintemberg in the 1930s and Charles A. Martijn in 1977, at which point I had identified a possible Dorset endblade (McCaffrey 1986).

The triangular endblade of heavily patinated beige chert or siltstone had been collected by Martijn from a site he discovered at Portage-du-Cap, on Île du Havre Aubert (fig. 18.3). The site, consisting of nine surface concentrations of lithic artifacts, was situated on a terrace rising 15 meters

above the Baie de Plaisance. Martijn’s surface collection also included flakes from quartzite beach cobbles, quartz *pièces esquillées*, and some finished artifacts and flaking debris of what appeared to be Ingonish Island rhyolite, a raw material that originated in Nova Scotia. Martijn’s field notes and photographs indicated that the site was severely damaged by erosion.

The chert endblade recovered from Portage-du-Cap resembles Dorset triangular endblades from Newfoundland, where Paleoeskimo occupation dates from about 2800 to 1300 B.P. (Renouf 1999). To date, however, no Dorset sites have been found south of Newfoundland. The only other instance of a well-documented Paleoeskimo artifact found in a southern context is the burin-like tool from the Goddard site in Maine (fig. 18.4). Situated near Blue Hill Bay on the central Maine coast, the Goddard site was occupied several times between the Middle Archaic and the Early Contact periods. Most abundant at the site are the remains of a Late Ceramic period occupation dating from approximately 1000



18.2/ Artifacts from the Landry Collection at the McCord Museum include: ivory artifacts (left), a slate endblade (middle) and two miniature soapstone lamps (right).¹ All objects shown at 1:1 scale.

to 500 B.P. During this period, the Goddard site appears to have encompassed a major village occupied during the summer and early fall with a primary subsistence focus on marine resources. The assemblages attributable to the Late Ceramic period are of particular interest because of the high frequency of exotic lithics and the presence of native copper. The diversity of lithic materials identified on the site supports the view that the occupants of Maine, Quebec, Labrador, and the Atlantic provinces participated in a wide-ranging exchange network during the Late Prehistoric period (Bourque 1994; Bourque and Cox 1981).

While examining the endscrapers collected at the Goddard site during work that had taken place in the 1950s, Steven Cox identified a Dorset burin-like tool:

The tool itself is made of chalcedony from an unknown source, and is a typical Dorset form with ground and polished faces and margins. Originally, the tool probably functioned as a graver in working bone, antler and ivory, but this particular specimen appears to have been unifacially retouched along a basal break, transforming it morphologically into an endscraper. Reworking of this sort is not characteristic of Dorset, and it is likely that

an Indian group reworked the tool into the more familiar endscraper form. (Bourque and Cox 1981:24)

Bourque and Cox (1981:25) suggested that this tool was most likely introduced to the Goddard site by way of exchange with aboriginal groups living to the northeast. They emphasized that no additional data exist to suggest that there had been an actual Dorset occupation at the site. They also pointed out the existence of other examples of previously unrecognized Dorset artifacts found in a southern context (Bourque and Cox 1981:24–25): “Smith and Wintemberg picture, but do not identify, two Dorset harpoon heads from a shell heap at Merigomish, Nova Scotia (Smith and Wintemberg 1929:plate 20:1, 2), as well as at least two other artifacts from the same area which may be Dorset (plate 20:4, 5)” [reproduced as fig. 18.5].

The Correspondence

The discovery of the Landry collection was certainly both surprising and intriguing. At last, there appeared to be substantial evidence of a Paleoeskimo presence south of the St. Lawrence River. Moreover, the



18.3/ Front and back views of Dorset endblade found at Portage-du-Cap (ChCl-1), Îles de la Madeleine

significance of the find had gone unrecognized because the collection had been packed and stored in the McCord Museum for decades. I quickly contacted Charles A. Martijn, an archaeologist and ethnohistorian then employed at the Ministère des affaires culturelles in Quebec, to share the news of what I thought might be a very important discovery.

To my surprise, Martijn informed me that I was not the first to ponder the significance of a possible Paleo-eskimo site on the Gaspé peninsula—the “elders” of Eastern Arctic archaeology had dealt with the issue long before me. Furthermore, they had left behind a stack of correspondence² on the topic, which Martijn forwarded to me. The letters had been sent to him in 1976 by William Fitzhugh, then a curator and chairman of the anthropology department at the National Museum of Natural History (NMNH), Smithsonian Institution. Fitzhugh’s note read as follows: “P.S. I’m also sending you some correspondence which I think would be interesting to you and your Quebec friends. Henry Collins dug the correspondence out for me and I find it a fascinating topic which should surely be investigated further. The possibility of Eskimo sites in the northern Gaspé would be extremely interesting. Someone really ought to check this out further, very carefully” (Letter from Fitzhugh to Martijn, January 12, 1976).



18.4/ Burin-like tool found at Goddard site, Maine, shown 1:1 scale

The letters, all dating to between March and October of 1952, were written by some of the most respected names in arctic research—Vilhjalmur Stefansson, Henry Collins, Elmer Harp, and Max Dunbar. They recorded their speculations and discussions about a “curious” collection of artifacts found in Newport, Gaspé. The first letter, dated March 30, 1952, was from Stefansson, an explorer and Arctic consultant at the Baker Library, Dartmouth College, to Henry B. Collins, a curator in the anthropology department at the NMNH. The letter began with an exciting invitation: “Evelyn and I got home from a week in Canada yesterday and brought a small parcel of specimens and the outline of a story, both intended for you, the trophies to be sent to you if you reply affirmatively on the basis of the story which now follows.”

The parcel of specimens Stefansson referred to were Paleo-eskimo artifacts supposedly collected in Newport, Gaspé, by a certain Captain Ambrose Landry, who lived in the region. Stefansson went on in the March 30 letter to relate the whole story, which he had heard from Duncan M. Hodgson, a wealthy Montreal stockbroker, McGill graduate, and member of the Field and Stream Club who had led expeditions to Africa. According to Hodgson, Captain Landry had found the specimens under a rock on the floor of a “stone igloo.” Apparently, he had discovered several of these structures by following an old road into an area of scrub maples near the town of Newport, located close to the southeastern tip of the Gaspé peninsula. Unfortunately, Landry had died leaving no notes or map to record his startling discovery.

Stefansson continued to relate the story he had heard from Hodgson:

An arrangement was made during a winter for a trip in to the site the following summer; but that winter the informant died. Through various reasons no proper search has been made since, but the impression still remains that the reported discovery is authentic: that there are several of these stone houses, that they are still standing, that the discoverer entered at least one of them, and that he found the specimens under the flags of a stone floor. (Stefansson to Collins, March 30, 1952)

Stefansson described the artifacts as “indubitably Eskimo,” and as “one of two things, the toys of an Eskimo girl child or the token gear made for a deceased woman for her use in the spirit world.” He then listed a miniature soapstone cooking pot, a miniature soapstone lamp, a polished stone, some bone implements, a blade, and a tooth. He had obtained the collection on loan from the Redpath Museum of McGill University, “to whom the specimens now belong.” Tentative plans for an expedition were laid out.

Hodgson is still eager to follow up and will, I feel sure, make all the necessary legal (if any are required) and physical arrangements for the search. His feeling is that the party should go prepared for a search of several days, though less may suffice, that the best time is October, when the leaves are off the trees. But his friend (and yours and mine) Max Dunbar wants to be in on this and has to leave for a year in Denmark next August; so the trials would then be in April or May. (Stefansson to Collins, March 30, 1952)

Maxwell (Max) J. Dunbar, a well-known figure in arctic research, was a graduate of McGill University and a member of the faculty from 1946 until his death in 1995. A marine biologist, zoologist, oceanographer, and medal-winning polar explorer, Dunbar designed the research vessel *Calanus*, the first Canadian ship specifically made for arctic marine research (Grainger 1995:306–307). It is not surprising, then, that Dunbar was anxious to join in the search for this remarkable site. Stefansson’s letter concluded with a formal invitation. He felt sure that Hodgson “would love to finance and lead a scientific junket to Gaspé” (Stefansson to Collins, March 30, 1952). Max Dunbar had expressed interest. Would Collins join them?



18.5/ Two Dorset harpoon heads (1,2) and other artifacts that may be Dorset from Merigomish, N.S.

Henry Collins’s response to Stefansson, in a letter dated April 10, 1952, was enthusiastic: “You hold out a tempting prospect in suggesting that I might join the expedition to the Gaspé. If the ruins can be found and there was a possibility they could be excavated or investigated, I would certainly be glad to go along.” Continuing in a more serious vein, Collins wrote: “The possibility of Eskimo stone iglus in the Gaspé peninsula is intriguing indeed. It would be a matter of real importance to determine whether Eskimos had once lived that far south. It would not be unreasonable, as Dorset cultural remains (but no houses) are found on Newfoundland.” Collins suggested that Stefansson show the specimens to Elmer Harp who “could tell you definitely if they are Dorset.” At the time, Elmer Harp was curator of anthropology at the Dartmouth

College Museum and an assistant professor of sociology. He was also a Ph.D. candidate at Harvard University.

We next pick up the thread of this story on May 23 with a letter Harp wrote to Collins after seeing the collection of artifacts:

I was very much interested when Stef brought over the small collection of artifacts from that site near Newport on the Gaspé, and I felt certain that you would like to know more about them too. The enclosed photograph is for your file . . . In order to give you a somewhat better idea of the specimens I have also enclosed my own brief notes and sketches (which I would appreciate your returning at some future time). (Harp to Collins, May 23, 1952).

Interestingly, the collection described by Harp is different from the one I had unpacked in the McCord Museum. Although Harp's notes and sketches are missing from the correspondence file, he listed the artifacts and their catalog numbers below the photograph. Another search in the McCord's accession books revealed that the objects described by Harp had indeed been accessioned into the Redpath Museum in 1939. The collection consisted of a miniature stone lamp, one complete and three fragmentary slotted bone knife hafts, three bone tool fragments, a boat-shaped steatite artifact, a bone or ivory wedge, a perforated tooth cap, and a smooth stone.³ Although a later letter mentions that Stefansson would give the artifacts back to Dunbar to return to Montreal, this collection has not yet been located.⁴

Harp offered Collins some tentative conclusions regarding the possible cultural affiliation of the artifacts:

My impression of this material, coupled with the meagre description of the stone houses, leads me to believe that it stems from comparatively recent Labrador Eskimo. It seems to be akin to Junius Bird's finds from Hopedale, but at the same time there is for me a suggestion of Dorset influence. I see this particularly in the bone hafts which have been slotted for side blades, and also in the basal socket (or open bed) of 4882-A. As I have noted on the cards, this latter specimen

appears to have one face missing, in which case I would suspect a true socket; on the other hand, perhaps that face of the artifact is only somewhat eroded and perhaps there was only an incised bed in the stem. At any rate, your greater experience will lead to more certain interpretation. (Harp to Collins, May 23, 1952)

Obviously impressed with the implications of this discovery, Harp went on to question Collins: "[T]his find from the Gaspé is most interesting. Am I correct in believing that it is the first authentic Eskimo material known from the southern shore of the Gulf of St. Lawrence." Harp expressed hope that a late summer trip to the Gaspé would be feasible, and offered to participate and help in some way or at least act as an observer should McGill University take the lead.

On May 28, 1952, immediately after receiving a copy of Harp's letter to Collins, Stefansson wrote to Max Dunbar at McGill University. He reviewed his discussions with Collins and Harp and enclosed a copy of Harp's letter describing the artifact collection.

As soon as Elmer [Harp] was ready to make a preliminary report [on the artifacts he had seen] we had a meeting at our house of the 8 or 10 most interested Dartmouth people, who proved most enthusiastic on the basis of Elmer's views. We feel there should be two "expeditions" to Newport, Gaspé, the first of only two or three just to locate the site; the second properly equipped, with rightly chosen members, to investigate the site. We feel that McGill, or at any rate Canadians, should be the leaders; but we at Dartmouth hope for the role of junior partners. (Stefansson to Dunbar, May 28, 1952)

In the last paragraph of this letter we learn that Dunbar planned to visit Stefansson at his home in Dearing, Vermont. Stefansson suggested combining this visit with a meeting to talk over the project with Duncan Hodgson, Harp, and some other interested Dartmouth people. He hoped that Collins would be able to make it, or alternatively, would share his views by letter.

In June 1952, a number of lengthy letters were exchanged between Stefansson, Collins, and Harp. Their discussions explored such topics as the occurrence of

stone structures at sites across the Arctic that might compare to the “stone igloos” of the Gaspé peninsula; the identification of arctic sites where artifacts were found under flagstones; the possibility that the Newport artifacts were actually located in a storage compartment under a sleeping platform; and the importance of expanding the initial Newport survey to include a broader area of the Gaspé and the Lower North Shore. In a letter to Harp on June 13, Collins explained, “Of more immediate interest, in connection with the Newport finds, would be a similar survey all around the Gaspe Peninsula, to see whether this southernmost of all Eskimo sites is an isolated phenomena or whether the Eskimos had at some time in the past actually occupied these coasts.”

Finally, in a June 17 letter, Stefansson wrote to Collins to explain that a meeting had been set up to plan the expedition: “As I think I mentioned in my last letter, the Dunbars will be here at Dearing July 12–13 and Sunday, July 13, a party from Hanover will join us to plot a Gaspe campaign. If only you could join us.” The July meeting proved to be the turning point in the saga of the elusive Landry site. In a letter from Harp to Collins, dated July 28, we learn the disappointing outcome of Dunbar’s visit:

Max [Dunbar] expressed a strong doubt concerning the authenticity of the site. First of all, it turns out that Landry, who is reported to have discovered the site and collected the material, was a member of the Bernier Expedition which wintered in Craig Harbour, southern Ellesmere Island, in 1922–23. He is believed to have brought some Eskimo material home with him, and Max suspected that out of this had grown either an innocent or humorous hoax, or else an innocently garbled report. (Harp to Collins, July 28, 1952)

The Bernier Expedition refers to one of the many arctic voyages undertaken by Captain Joseph Elzéar Bernier (1852–1934), who had a long and illustrious career as a seaman, explorer, entrepreneur and lecturer. He first went to sea at the age of fourteen, and eventually commanded over 100 sailing vessels on

voyages all over the world, including many trips to the Arctic. For example, between 1912 and 1917, Bernier made three voyages to Pond Inlet on Baffin Island, where he maintained a trading post until 1920. From 1922 to 1925, Bernier worked for the Canadian government patrolling eastern Arctic waters. During these voyages, he spent time at Craig Harbour, Dundas Harbour, Pond Inlet, and Pangnirtung. In sum, it seems clear that if Landry was on board for one of Captain Bernier’s expeditions, he most likely did have the opportunity to visit the Arctic (Dorian-Robitaille 1978; Fairly 1954; Marsh 1988).

Remaining optimistic, Harp expressed hope in his July 28 letter that the site would still be located: “If I can finish my thesis soon enough, I would still like to drive up to Newport and look around for a few days, although I couldn’t hope to gain much from interviews because my text-book Parisian French doesn’t stack up too well with the local dialect.” A final letter mentioning the Landry collection was written by Collins to Stefansson on October 9. It begins with Collins’ acknowledgment of uncertainty as to the “authenticity” of Landry’s find. He agrees that no further steps should be taken to pursue the project.⁵ The letter quickly moves on, however, to explore other research issues that had been raised in the course of discussions about the presumed Landry site—such as the nature and distribution of stone floors and stone habitations on sites across the Arctic. Although one small chapter in the history of arctic archaeology had come to a premature conclusion, Collins’ letter makes it clear that many chapters remained to be written, and there was no point in losing time before tackling other unresolved issues.

Postscript

Almost fifty years have passed since these letters were written. In the meantime, a number archaeological research projects have taken place in the Gaspé peninsula. For the most part, however, surveys and excavation work have been concentrated along the northern



18.6/ Aerial view of Portage-du-Cap site, Îles-de-la-Madeleine, Quebec

coast and in the western interior region. The record of prehistoric and early historic period occupation along the southern coast remains very poorly documented and relatively unknown. Although none of the sites discovered to the present time in the Gaspé peninsula have produced Paleoeskimo materials, the fact remains that many parts of the peninsula, including the area around Newport, have not yet been carefully investigated.

The situation on the Îles-de-la-Madeleine is quite different. In the summer of 1988, I began an archaeological survey program, sponsored jointly by the Municipalité des Îles-de-la-Madeleine and the Ministère des affaires culturelles in Quebec. Three seasons of fieldwork, from 1988 to 1990, on the islands demonstrated that this tiny archipelago has a much richer archaeological record than anyone had thought possible, especially considering the islands' distance from the mainland and their high rate of erosion (McCaffrey 1992, 1993). To date, more than thirty-six prehistoric sites, as well as two historic period occupations, have been identified.

Although excavations have yet to be carried out on the Îles-de-la-Madeleine, my limited test excavations and surface collections have provided many indices as to the age and cultural affiliation of certain

prehistoric occupations. Diagnostic lithic artifacts recovered from a number of sites suggest that the islands may have been occupied as early as the Late Paleoindian and Early Archaic periods (8000–6000 B.P.). Distinctive concave-based projectile points found on the surface of three sites compare well with similar specimens recovered from Prince Edward

Island and other parts of the Maritime Provinces. In addition, two sites have produced side-notched projectile points that most probably date to the Archaic period (6000–3000 B.P.). In general, however, this time period is not yet well represented on the Îles-de-la-Madeleine.

Finally, stemmed projectile points have been recovered from the surface and in test excavations at quite a few of the sites. This tool style was frequently found in association with fragments of ceramic vessels, indicating occupations dating to the Ceramic period (2500–500 B.P.). Charcoal samples from hearths on two of these sites, ChCk-1 and ChCl-18, returned dates of 1560 ± 60 B.P. (Beta-30215) and 1709 ± 100 B.P. (Beta-44550), respectively, supporting the Ceramic period affiliation.

During the 1988 field season, I paid particular attention to the careful investigation of site ChCl-1 at Portage-du-Cap in the hope of finding further evidence (in addition to the Dorset endblade discovered in 1977) to confirm a Paleoeskimo presence on the islands. Although lithic material was recovered from the surface of the site, no diagnostic Paleoeskimo artifacts were located. Moreover, the site was found to be completely disturbed due to a combination of wind erosion and all-terrain vehicle activity (fig. 18.6). Perhaps

the Dorset endblade was not brought to the site of Portage-du- Cap by Paleoeskimos but, instead, arrived on the islands as a result of exchange activities, much like the Dorset burin-like tool found on the Goddard site in Maine. Then again, the endblade may have been washed ashore in a dead or injured seal and carried up from the water's edge by Ceramic period occupants of the site. Nevertheless, the fact remains that the Îles-de-la-Madeleine, with their rich marine resources that included seal and walrus herds, would have been potentially very attractive to Paleoeskimo visitors.

Additional evidence indicating the southern presence of Paleoeskimos has recently come to light on the French islands of Saint-Pierre et Miquelon, off the southern Newfoundland coast. Preliminary archaeological work at the l'Anse-à-Henry site, on the island of Saint-Pierre (LeBlanc 2000), has produced evidence of occupation by both Groswater (2800–2100 BP) and Dorset Paleoeskimos (1900~1100 BP).

Conclusions

For a very brief period, planned research on the Landry site held out the promise that new frontiers in our knowledge of Paleoeskimo occupation in the Northeast would be explored. It should come as no surprise, then, to discover that the purported site was immortalized in the literature. In a 1939 publication entitled *Sur le peuplement de l'Amérique du Nord*, Aristide Beaugrand-Champagne, one of Quebec's pioneering archaeologists, alluded to the possibility of an Inuit site in the Gaspé region: "...during the French Régime, Eskimo could still be found on the North Shore from the Natashquan River region to the Strait of Belle Isle, and on the west coast of Newfoundland. I have heard that remains of Eskimo houses were found in the Gaspé peninsula a few years ago..." (Beaugrand-Champagne 1939:253, my translation).

Back at the McCord Museum, I have been tempted to put the artifacts from the "supposed Eskimo stone igloo" at Newport, Gaspé, back in a drawer with their original label. Perhaps a stimulating new debate would

arise when they are rediscovered in a few decades. Meanwhile, the elusive Landry site stands in the history of Eastern Arctic archaeology as a reminder of the many unanswered questions that await further research, and as a testimony to the innocence, enthusiasm, and freewheeling exchange of ideas and information (often across borders and disciplines) that characterized the elders' approach to research. We can only hope that arctic archaeologists always value these characteristics and never lose their sense of humor.

Acknowledgments

There are few archaeologists working in Quebec (or the wider Northeast for that matter) who have not benefitted, at one time or another, from the advice and assistance of Charles A. Martijn. I consider myself privileged to be a member of this group. I would also like to thank William Fitzhugh for inviting me to present this paper at the Elders Conference on the History of Archaeology in the Eastern Arctic held in 1993 at Dartmouth College. Barbara Lawson, Curator of Anthropology at the Redpath Museum, McGill University, and Calen Haak, Registrar at the Hood Museum, Dartmouth College, took time from their busy schedules to try and track down the missing part of the Landry collection. Finally, Bruce Bourque, Steven Cox, and Stephen Loring contributed valuable information and provided helpful comments on an earlier version of this paper.

Notes

1. Landry Collection artifacts as pictured in Figure 18.2 and their catalog numbers are: (far right) two miniature steatite lamps, ACC4829A, ACC4829B; (bottom right) bone tool fragment ACC4834; (middle) slate endblade ACC4828; (bottom left, L to R) two perforated ivory pendants ACC4831B, ACC4831A; (middle row, far left) perforated ivory pendant, ACC4831C; (middle row, 2nd from left) an ivory awl ACC4832A; (middle row, 3rd from left) perforated ivory pendant, ACC4832B; (top left) ivory bear with an incised skeletal motif (ACC4830). Not pictured is a small ivory object of undetermined function (ACC4832C).

2. This correspondence is located in the Henry B. Collins Papers, National Anthropological Archives, Smithsonian Institution. The letters cited in this chapter are listed below.

3. The catalog numbers of the artifacts listed by Harp but currently lost are: a miniature stone lamp (ACC4880); one complete and three fragmentary slotted bone knife hafts (ACC4882A, ACC4882C, ACC4883A, ACC4883B); three bone tool fragments (ACC4882B, ACC4883C, ACC4886); a boat-shaped steatite artifact (ACC4881); a bone or ivory wedge (ACC4884); a perforated tooth cap (ACC4885); and a smooth stone (ACC4879).

4. My search for the missing artifacts proved futile. Both Barbara Lawson, Curator of Anthropology at the Redpath Museum, McGill University, and Calen Haak, Registrar at the Hood Museum, Dartmouth College, attempted to track down the lost artifact collection. Our efforts were unsuccessful despite searches at both institutions.

5. In the 1950s, Graham Rowley (personal communication 1993) asked a woman living in Ottawa to visit the Newport region to see if she could learn more about the supposed Inuit site. According to Rowley, Margaret Lavender apparently spoke to Captain Joseph Bernier's nephew and conveyed to Rowley her impression that the Landry site was indeed a hoax.

Correspondence

The letters cited in this chapter are from the Henry B. Collins Papers, National Anthropological Archives, Smithsonian Institution, Washington, D.C. and include:

Letter from Vilhjalmur Stefansson, Dartmouth College Museum, to Henry B. Collins, Smithsonian Institution, dated March 30, 1952.

Letter from Wilfrid Bovey, Montreal, to Colonel Leon Lambert, Quebec Provincial Police, Quebec, dated April 8, 1952.

Letter from Henry B. Collins, Smithsonian Institution, to Vilhjalmur Stefansson, Dartmouth College, dated April 10, 1952.

Letter from Elmer Harp, Jr., Dartmouth College Museum, to Henry B. Collins, Smithsonian Institution, dated May 23, 1952.

Letter from Vilhjalmur Stefansson, Dartmouth College Museum, to Maxwell J. Dunbar, McGill University, dated May 28, 1952.

Letter from Vilhjalmur Stefansson, Dartmouth College Museum, to Henry B. Collins, Smithsonian Institution, dated June 1, 1952.

Letter from Henry B. Collins, Smithsonian Institution, to Elmer Harp, Jr., Dartmouth College Museum, dated June 13, 1952.

Letter from Henry B. Collins, Smithsonian Institution, to Vilhjalmur Stefansson, Dartmouth College Museum, dated June 13, 1952.

Letter from Vilhjalmur Stefansson, Dartmouth College Museum, (actually Dearing Farm, Vermont) to Henry B. Collins, Smithsonian Institution, dated June 17, 1952.

Letter from Elmer Harp, Jr., Dartmouth College Museum, to Henry B. Collins, Smithsonian Institution, dated July 28, 1952.

Letter from Henry B. Collins, Smithsonian Institution, to Vilhjalmur Stefansson, Dartmouth College Museum, dated October 9, 1952.

Letter from William W. Fitzhugh, Smithsonian Institution, to Charles A. Martijn, Ministère des affaires culturelles, Quebec, dated January 12, 1976.



Ben Ell, Deric O'Bryan's Inuit assistant from Coral Harbor, at Mill Island in Hudson Strait, with the ring seal he shot, July 1951

part 4

THE FUTURE OF THE PAST

Days of Future Past: Pluralizing Eastern Arctic/ Subarctic Archaeology

BRYAN C. HOOD

Time present and time past
Are both perhaps present in time future,
And time future contained in time past . . .
Footfalls echo in the memory
Down the passage which we did not take
Towards the door we never opened . . .

(T. S. Eliot, *Four Quartets*)

Retrospective Prologue

The main body of this chapter is a historical artifact. It consists of my 1993 Elders Conference contribution, only slightly modified in form and content. Since the theme of this volume is a history of eastern arctic archaeology, it seems appropriate to leave the text largely as it was first composed to preserve traces of that time past. The conference paper, however, became a catalyst for a more substantial critical research history in time future (Hood 1998b). Consequently, I have reformulated the conclusion of the 1993 paper into an epilogue anno 2001, with a commentary directed toward the current theoretical status of archaeology. Thus, the temporal convolutions of T. S. Elliot seem even more appropriate than before.

Looking at Ourselves (Part 1)

"Arctic research by its nature tends to be isolative."
(Maxwell 1976b:preface)

Compared with many other parts of the world, systematic archaeological research in the Eastern Arctic and Subarctic is relatively young. The research fraternity has been fairly small (and it has indeed been mainly a fraternity). In 1973, eight people participated



in the School for American Research (SAR) seminar on Eastern Arctic prehistory (Maxwell 1976c). Over the last twenty years our numbers have increased considerably; the invitation list for the 1993 Elders Conference at Dartmouth College contained the names of about fifty archaeologists, most of whom have ties to the Eastern Arctic or Subarctic. This demographic expansion has occurred on both the Canadian and Greenlandic sides of the Davis Strait.

As the demographic scale of a research community increases, so too does the mathematical likelihood for the development and dissemination of new ideas, facilitated by complex networks of interpersonal interactions and intergenerational debate. Curiously, though, the "subculture" of eastern arctic/subarctic archaeology has maintained an equilibrium in the face of changes in its surrounding theoretical environment. This is not to say that our subculture has been impervious to influence or change, but that our social boundaries have not been characterized by a high degree of conceptual permeability. It is this limited

permeability that is the subject of my commentary. The now three-decades-old debate over the merits of the once “new” or “processual” archaeology popped through a few breathing holes with a relatively modest and belated impact on our field, and the decade-old wrangling between processual and postprocessual archaeology has barely echoed off the ice floes. Some of the reasons for, and alternatives to, this state of affairs should be addressed as part of the historical retrospective and future prospectus that are integral to this volume.

The condition of restricted conceptual permeability is partly related to the social structure and history of our research community (cf. Crane 1972; Hagendijk 1990; Mullins 1973). Among the contributing factors are the limited number of institutions training graduate students and the recruiting patterns of these institutions. A quick perusal of the academic backgrounds of archaeologists working in the Eastern and Central Arctic and Subarctic indicates that four institutions have produced at least 50 percent of the Ph.Ds or equivalent: Calgary, Alberta, Michigan State, and Copenhagen. Moreover, certain other institutions have played important “feeder” roles for Ph.D. schools, providing B.A. and M.A. training for those continuing on to the doctorate, for example, Elmer Harp’s program at Dartmouth and the Memorial University of Newfoundland. I do not mean to imply that these eminent institutions produce academic clones, but as instruments of enculturation they tend to produce students with common outlooks. Furthermore, if there is a high degree of interaction between these few institutions, then a *system* of more-or-less shared values is nurtured within a generation or age cohort. At the intergenerational level, Ph.Ds from “school 1” become teachers at “school 2” where the outlook of “school 1” is transmitted indirectly to a younger generation of students who, ironically, may then go to “school 1” for graduate work and receive directly a second layer of “school 1” ideas.

Of course, the eastern arctic/subarctic network is not a seamless web of common discourse. For

obvious linguistic and cultural reasons, the research circles developed at Copenhagen and the University of Québec at Montreal have diverged substantially from the conceptual and methodological framework common to the anglophone Canadian and American research communities. Nevertheless, both of these circles perform enculturation functions for their milieus similar to the main centers of the anglophone world and have the same implications for conceptual impermeability.

The insular nature of our research communities is reinforced by practical necessity. Aspirants learn from their elders how to cope with the unique hazards of northern research. Furthermore, until recently (i.e., Maxwell 1985), essential knowledge was often transmitted as oral folklore rather than in a more widely accessible and synthetic written form. Consequently, arctic/subarctic fieldwork has perhaps been more dependent on apprenticeship to an experienced researcher than would be the case in other parts of North America. This results in “mentored” research circles that function as strong enculturation units, even when apprentices are recruited from outside the circle or the subculture. I need go no further than to cite my own initiation into northern research within William Fitzhugh’s Labrador archaeology family.

The social structure I have described may be marked by relatively closed systems of discourse and subtle power relations in which the research programs are driven by a limited number of individuals. Certain questions are judged to be worthy or unworthy of research, one interpretive framework is favored over another, and appropriate linguistic conventions are defined (cf. Foucault 1971; Kuhn 1970). The puzzle-solving agenda of “normal science” is set. While questions concerning the power and control of the agenda-setters are central to sociological analysis, I do not wish to impute Machiavellian intents. Instead, I prefer to emphasize the important unintended consequences that this community structure may have for narrowing the scope of the prevailing discourse.

Perhaps the situation as I have represented it is merely a historical artifact of the relatively young and undeveloped nature of the northern research community. Sociological analyses of other academic fields indicates that there may be “natural” stages in the life histories of research communities, each with its own consequences for innovation and the dissemination of ideas (Crane 1972). Maybe we should simply attribute our circumstances to the infancy of our field, carry on as usual, and let “nature” take its developmental course. Perhaps. But all research communities are actively constructed by intellectual agents who act to either maintain or change structures of discourse. Passive *laissez-faire* strategies tend to reinforce the status quo, and thus opportunities to open new doors and explore new passages may be missed.

For a successful adaptation to our future research environment, I think we need to break this stable equilibrium mode—a low-risk intellectual strategy—and engage in a modest adaptive radiation into new intellectual niches, some of which may seem, at first glance, to be dangerous habitats underlain by thin conceptual ice. In other words, perhaps our adaptive strategies

should become more generalist, theoretically speaking. I will make some suggestions for accomplishing this goal, but will begin with a respectful consideration of what I regard as the limitations embedded in the wisdom of my elders.

Looking Backward

Critiques of the status quo have an unfortunate tendency to engage in the rhetorically expedient strategy of tarring their opponents with the same brush. They often make it appear as if there is a homogeneity of opinion—a normative consensus—in the school they are attacking, despite the fact that this rarely exists to the extent that is implied. I do not wish to create straw people here, but a degree of gentle stereotyping is perhaps inevitable. It is difficult to divide any segment of archaeology into developmental stages without imposing artificial boundaries on ideas. It is even more difficult in northern archaeology, given the tenacity of traditional approaches into the present. Nonetheless, for the sake of expediency I will do so.

Until about 1970, the construction of culture-history was the primary goal of northern archaeologists

(although it is obviously still the major goal today). This is hardly surprising given the prevailing intellectual currents and the lack of basic culture sequences from most parts of our research domain. Unapologetic culture-history was quite appropriate. When Elmer Harp’s offspring entered the picture in the late 1960s, culture-history was conjoined with a conceptual structure based on Steward’s (1955) cultural ecology. There was an oblique rapprochement with the “new” archaeology, best exemplified by the SAR seminar (Maxwell 1976c), in which we see the influence of systems theory and quantitative analysis (fig. 19.1).



19.1/ Attendees to the SAR seminar were [rear, L-R] Albert Dekin, William Kemp, William Fitzhugh, Father Mary-Rousselière, Robert McGhee, Ronald Nash, [front, L-R] James Tuck, Moreau Maxwell, Elmer Harp, and William E. Taylor.

But the element of processual archaeology that seemed to *stick* best was ecological determinism. This was welded into a tentative alliance with a culture-historical approach that processualists would consider to be highly “normative.” Given the social context of the time, with the emergence of the environmental movement, it is not surprising that a concern with the relationship between culture change and climate change became primary (Barry et al. 1977; Dekin 1972; Fitzhugh 1972b, 1977b; McGhee 1972a; Trigger 1989:319-320). Of course, this concern was not strictly paradigmatic or sociologically driven, since climatic elements constantly and often unpleasantly impinge upon the awareness of field archaeologists in the north.

The alliance between normative culture-history and ecological determinism has dominated northern archaeology up to the present time. The result is an archaeology that understands cultural variability in largely functionalist technoeconomic terms or in a historical particularist framework as shared norms of behavior, or in an eclectic combination of both. Culture is either an epiphenomenon of adaptation or a superorganic entity explicable in terms of itself or of random historical events (e.g., McGhee 1976b:39, 1982b:74, 1983:23).

Nonetheless, there have occasionally been some alternative voices. Schindler (1985) launched an unappreciated critique of normative thinking, while Bielawski (1988:71-72) encouraged the exploration of new perspectives. Fitzhugh and Lamb (1985) backed away from the previously tight climate change/culture change models postulated for Labrador (e.g., Fitzhugh 1972b, 1977b). Jordan (1978) and Kaplan (1983, 1985) applied socioeconomic models to contact-period Labrador Inuit society, and Nagle (1984) used an economizing model to interpret Dorset lithic procurement and exchange in Labrador. Taylor (1967a) and Swinton (1967) planted the seeds for social and symbolic interpretations in their discussion of Dorset art and shamanism, and this was taken up in several subsequent papers (e.g., Fitzhugh 1985c; McGhee 1977; Plumet 1989a; Taçon 1983). Binfordian middle-range theory

and optimal foraging models have colonized our domain (Savelle 1984, 1987; Savelle and McCartney 1988; Stenton and Park 1994), and these seem to be easily assimilated into the prevailing conceptual framework. As far as the current archaeological preoccupation with middle-range theory is concerned, it is certainly important to acquire a better understanding of the formation of the arctic/subarctic archaeological record, but we cannot restrict ourselves to the adaptive causal factors that dominate this approach. Archaeological site formation is just as much a consequence of social variables as it is of technoeconomic variables (cf. Binford 1980 and Hodder 1982).

Indeed, what is most conspicuous in its absence from northern archaeology is a sense of *social process* (but see Fitzhugh 1984b; Grier and Savelle 1994; Nagle 1984). In calling for a pluralization of eastern arctic/subarctic archaeology, I am first and foremost advocating the development of a *social archaeology* for the north. This would respond to Elmer Harp’s criticism of attempts to apply the “new” archaeology program to the Arctic and his preference for an archaeology of human communities:

[T]o the extent that we think solely in such statistical and materialistic terms, the fundamental human nature of our quest may be diminished, if not lost altogether. . . . In the same vein, archaeology’s current fascination with systems theory is also somewhat antisocial insofar as it obfuscates humanistic values. . . . As archaeological taxa, cultures can also be usefully treated as systems. However, the burgeoning adoption of this term “system” seems to imply a teleological sense of sociocultural purpose and integration which is by no means inherent in most human affairs. Individual drives still motivate most human behavior, albeit this behavior is fundamentally conditioned by cultural norms, but in few societies do we find a degree of sophistication which can fully comprehend the intertwining networks of social, cultural, and ecological relationships and conceptualize them in terms of systems theory. . . . Therefore, I plan to operate on a level below such systems, and, given our limited means of remote sensing, aim for a view of communities and people. (Harp 1976a:119)

Looking Forward

One option for building a social archaeology is to explore some of the ideas expounded by so-called postprocessual archaeologists (e.g., Hodder 1985, 1986; Shanks and Tilley 1987a, 1987b). In order to avoid the now tired and frequently unpleasant polemics of this debate, I will sidestep critique and accentuate the positive benefits of rethinking our research domain in these terms. To my mind, one of the most important potential impacts of postprocessual perspectives on northern archaeology is in reorienting our concepts of culture. Rather than viewing culture as an adaptive mechanism or as shared ideas—cultural frameworks in which people are either passive adaptive automatons or clones of shared normative mental templates—cultural behavior is viewed in terms of social strategies played out through the reciprocal relationship between *agency* and *structure*. My perspective adopts the structuration theory of Anthony Giddens (1979, 1984).¹

For Giddens, “structures” consist of rules and resources. Social behavior is rule-governed, although not in the sense of normative behavior. Rule following may be situationally contingent, generating variable rather than standardized behavior. On some occasions, humans may violate the rules or manipulate them in their own self-interest. On other occasions, their actions may adhere more closely to cultural norms. Resources consist of material, social, and symbolic elements. By material resources I mean technology and subsistence items; social resources include kinship relations and labor organization; and symbolic resources are comprised of material symbols and ideological elements.

Humans engage in intentional social strategies within a framework of cultural meanings given by the rule system. The scope of these strategies is constrained by structures, but structures can only be reproduced or transformed through human action. Sometimes structures place strong limitations on the range of actions, while in other cases the arrangement of structures provides windows of opportunity for the

implementation of novel strategies. In the case of northern societies, for example, we must acknowledge that material resource structures may impose powerful constraints on action. Nevertheless, in some situations the manipulation of different combinations of material, social, and symbolic resources provide opportunities for the development of new strategies and structures.

I think that an approach that treats social action as patterned, but historically contingent, can help us balance the reality of material constraints with the need to explore the generative role of agency, social structure, and ideology. This approach can partially accommodate the calls for historical particularism (e.g., McGhee 1982b:79) while avoiding excessive relativism by situating historically contingent action within: (1) economic structures that exhibit at least limited cross-cultural consistencies, and (2) social and ideological structures that reproduce local historical traditions. Consequently, moderate forms of postprocessual archaeology are less antithetical to some aspects of traditional northern archaeology than are the more extreme elements of Binfordian processualism.

A corollary of the concern for social strategy is the recognition of power relationships. It is not so much that power struggles are a “universal” feature of social life, but that relations of autonomy/dependence are; these relations constitute the dialectic of control in a society (Giddens 1979:6, 88–93, 149). The question is, How do domination/resistance relationships arise within this dialectic and how do these relationships lead to inequalities in access to material, social, and symbolic resources (cf. McGuire and Paynter 1991)? Structuration theory thus provides a useful framework for conceptualizing the emergence of, and resistance to, social hierarchy. An important locus in the negotiation of social power is the construction of gender relations (Gero and Conkey 1991). Aside from some discussion of male and female work areas (McGhee 1979:52–55), northern archaeology has yet to be engendered (but see Cabak 1991, Gullason 1999). Structuration concepts may contribute to doing this.

There is no reason why the processes discussed above could not operate and be identifiable in some contexts studied by northern archaeologists. Although material resources may often be hard to accumulate and control in unpredictable arctic/subarctic environments, social and symbolic resources—involving much contrived behavior with material culture—may turn out to be a primary vehicle for the playing out of power relationships (e.g., Dorset shamanism and art production). Thinking about culture in this manner may aid us in perceiving *differences* between the ethnographic present and the past rather than in continually reproducing ethnography in prehistory (Wobst 1978).

The practical archaeological implication of postprocessual approaches is that the material culture excavated by the archaeologist cannot be viewed as a direct reflection of behavior. Social strategies may use material culture ideologically to *misrepresent* rather than mirror social relations (Hodder 1986:2-3, 61-70; Miller and Tilley 1984:13-14). Furthermore, material culture can be used to create or channel social action. Material culture, therefore, constitutes a signification system that communicates meaning in complex ways. Whether or not it can usefully be characterized as a “text,” material culture does exhibit text-like properties in the sense that, as a sign system, it is subject to multiple interpretations by different “readers,” including those from the past as well as those from the present and the future. A more pluralistic archaeology would provide space for exploring these alternative viewpoints.

The call for pluralism should not, however, be taken as an endorsement of unrestricted relativism. Postprocessual archaeologists take ambiguous stands on how interpretations can be evaluated. Many postprocessualists regard pattern recognition and the assignment of meaning to patterns as largely theoretically predetermined, and they proceed by intertwining data and interpretation in a repeated hermeneutic spiraling (e.g., Hodder 1991). Despite remarks that “. . . data represents a network of resistances to theoretical appro-

priation” (Shanks and Tilley 1987a:104), postprocessualists provide few methods or strategies for evaluating the accommodative fit of interpretations or for reducing error. Curiously absent from this process is the crucial inferential step of understanding archaeological material as an *archaeological record* from which behavioral patterns must reliably be inferred prior to any interpretation of cultural meaning. This step involves so-called middle-range theory. Although I do not accept the thrust of his program, I must agree with Binford (1981, 1982) that without an understanding of how the archaeological record was formed we cannot adequately describe or identify the meaning of observed patterns. Consequently, without some form of middle-range theory we have no grounds for determining the extent or reliability of model accommodation, whether our interpretive frameworks are adaptationist and positivist, or Marxist, (post) structuralist, and hermeneutic (see also Saitta 1992).

From Theory to Practice

How do these esoteric notions relate to the actual practice of northern archaeology? All I can hope to do here is point to a few areas where this approach might make a difference when interpretations are being constructed. In doing so, I will temporarily bracket off the middle-range epistemological questions noted above.

The only past context in which we can actually see agency/structure in operation (or at least have reliable grounds for inferring elements thereof) is in historical archaeology. A useful illustration is eighteenth-century Labrador Inuit society (Jordan 1978; Kaplan 1983, 1985; Taylor 1974, 1976). The Labrador Inuit communal house leader, or “big-man,” created and sustained his position by manipulating a range of material, social, and symbolic resources. His ability to attract a large coresidential group of followers and acquire multiple spouses increased his household’s productive potential. The leader’s control over access to, and distribution of, European goods

represented a source of power within the settlement and possibly beyond. Some leaders and their spouses were also shamans (Taylor 1989), thus giving them control of another position based on material and social relationships that could potentially be reinforced by the manipulation of symbolic resources. There is some debate as to whether these processes were marked by significant changes in Labrador Inuit value systems towards European concepts of private property or whether the processes involved the intensification of traditional social relations (Richling 1993). Either reading gives us at least a superficial glimpse into Labrador Inuit social strategies. Historical scholarship can also provide us with fragmentary biographical insights into the careers of significant eighteenth-century Inuit figures, such as Tuglavina, a prominent Inuit leader and trader, and his one-time wife Mikak (Taylor 1979, 1983–1984). Besides modeling Inuit socioeconomic strategies during the contact period, such information can also provide insights into gender relations and the agency of Inuit women (Cabak 1991).

The treatment of Labrador Inuit leaders and other community members as social agents does not imply methodological individualism. Rather, their agency must be seen as embedded in a range of broader structures, including Inuit kinship and ideological systems and economic and ideological relationships with Moravian missionaries and European fur traders. European groups were also composed of agents operating within their own structures, which embodied constraints and opportunities that were different from those enveloping the Inuit. Kaplan's (1983:339–375) description portrays these conflicting interests and agendas well, although the implications of the intersecting power relationships are not fully explored. This angle is taken up more overtly in Loring's (1998) research on different Labrador Inuit responses to the Moravian missionaries—specifically, the strategies of *resistance* employed by some Inuit groups in the face of European domination.

In prehistoric research, we face the obvious problem that structuration processes are not directly visible in the archaeological record and can only be inferred analogically. However, this apparent invisibility should not lead us to the conclusion that social strategies can be ignored in prehistory. In the following paragraphs, I show how the structuration framework can provide useful interpretive insights for a specific prehistoric context: the Maritime Archaic of Labrador.

Lithic Procurement in the Labrador Maritime Archaic

The Maritime Archaic Indians inhabited the central and northern coasts of Labrador from at least 7500–3500 B.P. Their initial notoriety among archaeologists was based on their maritime adaptation, mortuary ceremonialism, and long distance trade systems involving Ramah chert (Fitzhugh 1975, 1978a; Tuck 1976a). During the 1980s, Smithsonian researchers revealed a developmental trend in Maritime Archaic community patterns. Prior to 6000 B.P., these Indians used small single-family pit houses, sometimes arranged in groups of two or three. After this time, the pattern shifted toward increasingly larger rectangular dwellings segmented into individual household compartments. This trend culminated between 4000 and 3500 B.P. when segmented longhouses ranged up to 80 meters in length and probably accommodated seasonal aggregates of 50 to 100 people. All these elements suggest a culture that was developing increasingly “complex” social practices (Fitzhugh 1984b, 1985c; Hood 1995).

Many of the inferences concerning Maritime Archaic social relations have been derived from prominent features such as houses and burials. But we can also tap the interpretive potential of more mundane aspects of material culture, such as the organization of lithic procurement. A key element of Maritime Archaic lithic technology was the material known as Ramah chert. The chert was available only at the extreme periphery of the Maritime Archaic world in Ramah

Bay and adjacent bays in northernmost Labrador (Gramly 1978; Lazenby 1980). This inconvenient positioning required new organizational arrangements to procure and distribute the material among Maritime Archaic groups, which were spread across at least 600 kilometers of the northern and central coasts. Changing patterns of Ramah chert and local raw material usage may therefore point to significant transformations in Maritime Archaic social relations.

The earliest Maritime Archaic sites (ca. 7500 B.P.) at Hamilton Inlet on the central coast contain high frequencies of local quartzites and no Ramah chert. From 6000–5000 B.P., these sites exhibit a great quantity of local vein quartz, with lesser amounts of slate and local quartzite. They contain a small amount of Ramah chert debitage and varying percentages of finished tools made from Ramah chert. Sometimes projectile points were fashioned from purple cherts thought to derive from interior sources (Fitzhugh 1972b, 1975, 1978a; Lazenby 1984).

Further north in the Nain region, sites predating 6000 B.P. contain bifaces of local quartz and Ramah chert, abundant quartz debitage, and a minimal amount of Ramah chert debitage. Later sites (6000–5000 B.P.) exhibit a variety of materials. Local quartz predominates in the debitage and was used for bifaces, scrapers, and bipolar cores, while Ramah chert was frequently employed for bifaces and often comprises up to half the debitage (Fitzhugh 1978a; Lazenby 1984). Non-local chert from the Cape Mugford area to the north was often used for endscrapers and slates were employed for ground stone implements such as projectile points and celts. Overall, Ramah chert use seems to increase between 5000 and 4000 B.P.

For the early-middle Maritime Archaic, I suggest that two *technological* systems for organizing lithic procurement and use operated simultaneously: (1) an opportunistic system for acquiring local poor-quality raw materials (quartz, quartzites), which were used primarily for expedient activities (e.g., simple flake tools), and (2) a more structured system for procuring

nonlocal high-quality materials (cherts, slates), which were used for a limited range of specialized formal tools (e.g., projectile points, endscrapers, celts). Paralleling these technological organization systems were two *social* strategies for raw material use: (1) an opportunistic local procurement system that promoted high individual *autonomy* in lithic resource acquisition, and (2) a more structured system for procuring nonlocal materials that implied potential *dependency* relations on distant others if the materials were acquired through exchange. It follows from this contrast that social strategy (1) may have permitted individuals to *resist* the dependency relationships embedded in strategy (2).

During the late Maritime Archaic (4000–3500 B.P.) this picture changes dramatically. Ramah chert becomes highly abundant and is used almost exclusively for flaked stone tools, even at Hamilton Inlet, which is 600 kilometers south of the Ramah chert sources (Fitzhugh 1972b, 1975, 1978a; Lazenby 1984). Local raw materials are of minimal significance. This pattern indicates the large-scale transportation of prodigious amounts of Ramah chert. There are two alternative delivery systems, each with different social consequences: exchange and direct procurement.

If large quantities of Ramah chert were acquired through long-distance exchange systems, this situation might imply the emergence of strong interpersonal and/or intergroup dependency relations. Local raw materials were available but were not used to any great extent, so the procurement autonomy option pursued in earlier times was no longer implemented. This high degree of dependency on Ramah chert was not technologically determined but socially contrived. I suggest that the value of Ramah chert was largely ideological and that the negotiation of social relations and the construction of social identity in the late Maritime Archaic was bound up with participation in lithic exchange systems and their associated rituals. This process may have been linked to the emergence of status competition between individuals at longhouse

aggregation sites and the maintenance of alliance systems between regional groups positioned along a linear coastal social network.

An interesting component of these changes in lithic procurement systems and social strategies is their possible connection with the structuring of gender relations. Gero (1991) observes that the linkages between gender and lithic procurement and production systems are bound to be highly variable. Nevertheless, she suggests that we might at “minimum” monitor women’s involvement in lithic technology via the use of local raw materials and expedient tools found in domestic contexts (Gero 1991:176, 180). During the early-middle phases of the Maritime Archaic, it is these local materials (quartz, quartzite) and expedient tools (utilized flakes) that predominate in the assemblages. The nonlocal materials (Ramah and other cherts) are associated with curated tools, such as projectile points, which may (or may not) be associated with men’s labor “in the field,” and with endscrapers, which may (or may not) be associated with women’s domestic labor. In any event, there is a strong distinction between domestic activities/local raw materials/expedient tools and “field” activities/non-local raw materials/formal curated tools. This may imply some gender-linked differences in raw material use.²

During the late Maritime Archaic, both domestic and “field” activities were permeated by the use of Ramah chert. This merging of activity realms within a single raw material system could imply a shift in how lithics articulated with the construction and maintenance of gender relations. Specifically, it may point to a broader distribution between men and women of a socially valued material and a closer linkage between the negotiation of gender relations and the social and ideological components of Ramah chert exchange. Although this vague proposition requires further elaboration, it highlights the possibilities for the creative use of seemingly mundane material culture to explore social processes.

The second Ramah chert acquisition strategy is direct procurement. Fitzhugh (1985b:50) suggests that a large, late Maritime Archaic settlement in northern Labrador with as many as twenty-seven longhouses may have been a repeatedly inhabited, seasonal staging camp used for the direct procurement of Ramah chert. Entire central coast groups may have relocated to this camp north of the tree line for brief summer chert provisioning forays and caribou hunting. An interesting implication of this “expedition” model (pointed out to me by Fitzhugh) is that direct procurement could be seen as a social strategy to *resist* the dependency relations involved in long-distance exchange networks. This resistance would operate at the level of group autonomy through mobility strategies rather than through individual autonomy in procuring local materials.

Social Construction of Space in the Labrador Maritime Archaic

Another area where a structuration approach may provide insights is the social construction of space. The spatial relations of northern hunter-gatherers are generally seen as determined by environmental and subsistence variables. But landscapes are also culturally constructed, imbued with symbolic significance by ideological structures (Hood 1988). In the Labrador Maritime Archaic the social construction of space can be theorized on two levels: regional and intrasite.

At the regional level, it is interesting to note that despite the presumed seasonal mobility of Maritime Archaic groups, much of their ritual activity seems to be aimed at contriving “place.” By this I mean they established and legitimated group claims to particular places by modifying them with visible material culture (longhouses, burial mounds, cemeteries). These places were then connected by constructing marriage, exchange, and other alliances over space. There is a tension here between the seasonal mobility required by strong environmental constraints on subsistence procurement and the definition of more localized

social and ideological landscapes that may have helped anchor emerging corporate group identities (as suggested by longhouse coresidential units). The intensive seasonal investment of energy and symbolism in the built environment of particular places counteracted the mobility constraints and sustained the complexity processes that otherwise tend to be associated with sedentism. This is interesting since sedentism is often seen as a prerequisite or a critical concomitant of social complexity (Price and Brown 1985:11). During the late Maritime Archaic, these place-making processes may also have been related to the social dynamics of Maritime Archaic and Pre-Dorset boundary relations (Fitzhugh 1984b; Hood 2000).

At the intrasite level, longhouses may be seen as both “reflecting” Maritime Archaic social structure and as a vital element in *creating* that structure. I suggest that the spatial logic of the longhouse conveyed an important social tension within Maritime Archaic society between the autonomy of individual household units and the collective organization of emergent corporate groups. Maritime Archaic subsistence required seasonal mobility, which would promote fissionary tendencies, flexible autonomous social units, and temporary settlements. The segmentation of longhouses into multiple compartments comprising individual household floors was a means of spatially encoding household autonomy. Yet these individual social modules were incorporated into a larger collective unit by the overall structure of the dwelling. It was the longhouse structure as a whole that *physically created* a corporate unit and gave that unit a temporary (seasonal) material existence through the organization of domestic space. The longhouse also helped create an ideological fiction of collectivity in a social world that otherwise tended towards seasonal fragmentation. Thus the structure of the material world helps produce and reproduce forms of social action and belief; material culture is not just a passive reflection of social structure.

These examples show how a structuration approach can lead to new ways of looking at our data. They are tentative and incomplete. They also lack treatment of middle-range inference problems that are crucial to accurate description and identification of spatial patterning in longhouses, for example. But they do demonstrate that postprocessual approaches can provide some meaningful direction to concrete research problems.

Looking at Ourselves (Part 2)

The meaning of the past has to be inserted into the present through the medium of the text. So there is no meaning outside the text . . . The act of writing always presupposes a politics of the present, and such writing is a form of power. (Tilley 1989b:193)

One of the prominent jargon concepts in postprocessual archaeology is “text.” One can debate whether or not the text metaphor has much utility for interpreting material culture in the archaeological record, but it does have another important implication. The text metaphor implies that archaeological interpretation occurs solely through the medium of writing—in other words, text production (Tilley 1989a, 1989b, 1990). Archaeological text production is embedded in the social relations of the discipline and in the discipline’s encompassing sociocultural context. Thus all interpretation/writing is a product of the present (although postprocessualists vary in the degree to which they believe the past is independent of or constrains the text). Postprocessual archaeologists advocate greater critical consciousness in the writing process and the construction of archaeological texts that break with narrative convention. In other words, they propose a new aesthetics of archaeological text production.

Part of this new aesthetics, and particularly important for the present sociopolitical context of northern archaeology, concerns the *power effects* of our texts (Tilley 1989a, 1989b, 1990), specifically, who controls the construction/writing of archaeological texts? Who exerts authority over the form and content of our writ-

ten representation of the past? To phrase this in terms of structuration processes, how does the dialectic of control work itself out in the modern world of relationships between archaeologists (writers/producers), their readers/consumers, and the objects of archaeological knowledge (aboriginal people and their heritage)?

Up to this point, archaeological texts have been the product of "experts." Our texts consist of site reports and other technical documents incomprehensible to northern residents, both conceptually and linguistically (I include the present document in this category). Another means of pluralizing northern archaeology would be to break the monopoly of the experts over archaeological text construction by inviting northern peoples to participate actively in the creation of texts. Their role would be transformed from objects of knowledge and passive readers of a past composed by others to active writers of their own past in forms/genres of their own choosing as well as in their own languages. This will surely lead to conflicts with archaeologists over the "correct" interpretation of the past, yet this is an unavoidable issue that has already generated rancor (McGhee 1989). Resolution of the problem requires pragmatic negotiation of the distance between the meaning frames of archaeologists and those of aboriginal people (Anawak 1989; Bielawski 1989), as well as acknowledgment of the history of unequal power relations between the two.

Acceptance of joint authorship of the past may also contribute to the pluralization of northern archaeology, not only by opening it up to previously subordinate voices (Hodder 1991:14–16), but also by changing the social structure of our research community. Drawing First Nations people into the research community as active writers/producers and through technical training as field archaeologists (e.g., Andreassen 1988:15–16; Bielawski 1989:232) will allow their agency to change the present structure of the archaeological subculture, thereby transforming the social relations that generate the conceptual impermeability I noted at the outset.

Epilogue 2001

The skeptic is always playing on the fear that unless we achieve finality we have not achieved anything. (Bernstein 1983:69)

The preceding commentary engaged in some limited sociohistorical reflections on the links between our archaeological generations and considered how the social structure of our subculture conditions continuities and discontinuities in our thought. It also emphasized the consequences of our standpoint as archaeological writers in the present for our representations of the past and for the future development of our research community. I suggested that some aspects of postprocessual archaeology could provide useful insights into both the interpretation of the past and the analysis and improvement of the present social context of northern archaeology. Pragmatic engagement with a range of ideas from outside the dominant conceptual framework could contribute to pluralizing eastern arctic/subarctic archaeology.

During the past decade, much of the impetus for change in northern archaeology has come from forging new working relationships with aboriginal groups (e.g., Webster and Bennett 1997). Field schools have been set up at Igloolik and Iqaluit (Stenton and Rigby 1995), and archaeological research initiatives have been sponsored by institutions such as the Avataq Cultural Institute in arctic Quebec, the Torngâsok Cultural Center and the Innu Nation in Labrador (Loring 1995), and local museums in Greenland. Aboriginal groups have co-managed the archaeological components of environmental impact projects (Hood 1998a). Parks Canada sponsored an oral history and archaeology project with the community of Arviat that was aimed at incorporating Inuit traditional knowledge (Henderson 1997). Museums are also changing to meet the needs of northern First Nations (Issenman 1991). Finally, the establishment of Ilisimatusarfik (the Greenland University) and the Greenland Research Center (SILA) at the Danish National Museum (Grønnow 2000), as well as work toward establishing a University of the Arctic

add important new dimensions to northern education and research.

In recent years, steps have also been taken toward conceptual shifts that open space for a social archaeology for the north. McGhee's (1996) popular book traverses many aspects of Paleoeskimo societies. In more explicitly theoretical work, much is couched in "processual-social" terms, focusing on the adaptive or economic aspects of societies: resource structure and territoriality in Dorset-Thule interaction (Friesen 2000); "accumulator" whaling captains and Thule organization of production (Grier 1999); substantivist analysis of Inuit household economy (Henshaw 1999); scalar stress and the development of Thule social organization (Friesen 1999); interpretation of the transition between Pre-Dorset and Dorset as a shift between forager and collector organization related to either the accumulation of environmental knowledge (Nagy 2000a) or communal walrus hunting (Murray 1999); and style, material exchange, and interaction in the Dorset (Odess 1998). Other contributions reflect influences from postprocessual archaeology, such as the application of practice theory to regional variation in the Newfoundland Dorset (Leblanc 2000) and the construction of social difference in Thule (Whitridge 1999). Social interpretation of Dorset art continues and extends the original shamanism-related framework (Arsenault et al. 1998; LeMoine et al. 1995; McGhee 1996; Plumet 1997; Sutherland 1997; Taçon 1993). Park (1998) provides a critical assessment of the methodological requisites for inferring Thule social organization. These newer contributions reflect increasing diversity in theoretical and methodological frameworks and are signs of the hoped for pluralization of archaeological discourse.

One point in the preceding presentation that requires an epilogic comment is my tendency to frame the discussion in terms of the debate between processual and postprocessual archaeology. Although perhaps relevant in 1993, that debate is now largely dead and little useful purpose is served by adhering to

these polemical categories. Such paradigmatic positioning tends to result in one or another form of "normal science" thinking, cutting up knowledge into closed intellectual compartments, territories defended by their own theoretical identity politics. Instead, a pluralistic archaeology would regard knowledge as a complex landscape or network, with peaks or areas of high network density representing central tendencies in thought ("paradigms"), with many points of overlap and contact, but also areas of incongruence and contradiction. Rather than situating ourselves in the areas of central tendency (normal science), it behooves us to explore the peripheries and overlaps, since this is where much of the interesting conceptual development may occur (Galison and Stump 1996).

To use another analogy, that of the internet and the programming language Hypertext (Edwards 1994; Landow 1997), we can enter the knowledge network in many different places, navigate links along routes of interest, disrespecting traditional paradigm boundaries and constructing knowledge as "local wholes" that do not require downloading entire paradigmatic formulas (cf. Barker 1998:29-31; Wylie 2000:231). For lack of a better term I would call this approach to pluralism "heterodox intertextual" archaeology—heterodox in the sense of struggling against normal science and intertextual in that knowledge is seen as a complex network of interlinked texts rather than paradigm boxes. One of the key areas where such thinking needs to be applied is in breaking down the nature/culture dualism (see Ingold 2000) that lies behind the processual/postprocessual opposition and many aspects of arctic archaeology. But this is a challenge for another day.

To conclude, pluralization means exploring divergent passages and opening new doors. A pluralized northern archaeology would embody multiple and probably conflicting perspectives, both theoretical and cultural. There would be no unified science, no common set of goals, and few standardized methods. Archaeological experts would relinquish some of their

present near-exclusive control over excavating and writing the northern past. Yet, contrary to appearances, this pluralization is not an abandonment of the field to hyperrelativism. It is a recognition that interpretation in northern archaeology is situated within a complex network of theoretical and social interests and that knowledge must be constructed as local wholes within that network. Even if many problems of social archaeology prove to be empirically intractable, we should not fall prey to the skeptics' demand for interpretive finality. This would severely limit the scope of pluralization and condemn northern archaeology to sit in perpetuity on the lowest rungs of the ladder of archaeological inference. Nonetheless, we are still in need of much "basic research" on the culture-history of the north, as pioneered by Elmer Harp and other elders and near-elders. There is much that we do not know. Ambitious conceptual meanderings will remain ungrounded and perhaps irrelevant until more data are collected and transformed into useful evidence. But without ambitious conceptual schemes, our data can only whisper softly in the arctic night.

Notes

1. While I still view structuration theory as one of the best approaches to theorizing social action, I do not advocate slavish adherence to it. I append Giddens's own *caveat*:

The concepts of structuration theory, as with any competing theoretical perspective, should for many research purposes be regarded as sensitizing devices, nothing more. That is to say, they may be useful for thinking about research problems and the interpretation of research results. But to suppose that being theoretically informed—which is the business of everyone working in the social sciences to some degree—means always operating with a welter of abstract concepts is as mischievous a doctrine as one which suggests that we can get along very well without ever using such concepts at all. (Giddens 1984:326-327)

2. This interpretation verges on, but does not accept, the public/male and domestic/female dualism that is rejected by feminist anthropologists. I risk reproach for the sake of conveying the general point of engendering the past.

The 1992 Permitting “Crisis” in Eastern Arctic Archaeology

JAMES W. HELMER
GENEVIEVE LEMOINE

In the fall of 1991, the coauthors of this chapter¹ submitted a research grant proposal to the Social Sciences and Humanities Research Council (SSHRC) of Canada. In our submission, we outlined a three-year program of research into the Late Dorset (ca. 1500–1000/500 B.P.) occupation of Little Cornwallis Island in the Canadian High Arctic (Helmer 1989, 1991, 1996). The archaeological goals of the project included a detailed culture-historical analysis of three large Late Dorset village sites comprising, between them, some 248 discrete cultural features; an investigation of the inter- and intra-feature and site artefactual and faunal assemblage variability of the three sites; and a comparative study of Late Paleoeskimo settlement mobility strategies. Our proposal also included, as primary objectives, the training of graduate and undergraduate university students and northern residents and working toward improving community relations (Helmer and LeMoine 1991). In particular reference to this latter concern, our proposal stated explicitly:

Aboriginal Canadians are increasingly expressing their general dissatisfaction with non-native “scientific” reconstructions of their past. Native groups are also demanding the right to interpret their traditional history as they perceive it. It is essential that Canadian archaeologists respond positively to this movement before we are ultimately denied the access to the archaeological record that we have taken for granted for so long.

Happily, our grant submission was successful. On the downside, however, our predictions about the consequences of archaeology’s collective failure to



explicitly acknowledge indigenous concerns about the practice of archaeology in the Far North were to prove far more accurate than even the most pessimistic among us had reason to expect. In early June 1992, a mere two weeks before we were scheduled to ship our supplies and equipment north for the upcoming field season, we were informed that the Hamlet of Resolute Bay Council had denied approval of our excavation permit.

In this chapter, we will describe in greater detail our “permit crisis” during the summer of 1992 and the impact it had on our work in that and subsequent field seasons. We will articulate some of the perceptual difficulties that we, as practicing archaeologists, currently face in the North, and summarize what we perceive to be some of the implications this situation has for future archaeological research in this region.

The Permit Crisis

The permitting process in place in the Northwest Territories in 1992 required that we submit a permit application, including a one-page, plain English summary of our proposed research, to the Prince of Wales

Northern Heritage Center. From there, applications were forwarded to the appropriate local communities—in our case, the Hamlet of Resolute Bay on Cornwallis Island. Applications had to be approved at both levels in order for a permit to be issued.

Our original plans for fostering greater community involvement in local archaeological research were spelled out clearly in our research proposal. We specified that we would offer a college-level credit course in archaeological field techniques to eligible Northern students, conduct a series of illustrated public lectures to the community of Resolute Bay, organize a mid-season visit to Little Cornwallis Island by members of the hamlet council and community elders to discuss work in progress, and prepare a museum kit summarizing the results of our investigation of the Late Dorset settlements on Little Cornwallis Island that would be kept in Resolute Bay. By actively pursuing these objectives we hoped to establish a working relationship with the community and raise people's awareness and understanding of the aims and objectives of the discipline of archaeology (Helmer and LeMoine 1991).

The “Unfolding” of the 1992 Season

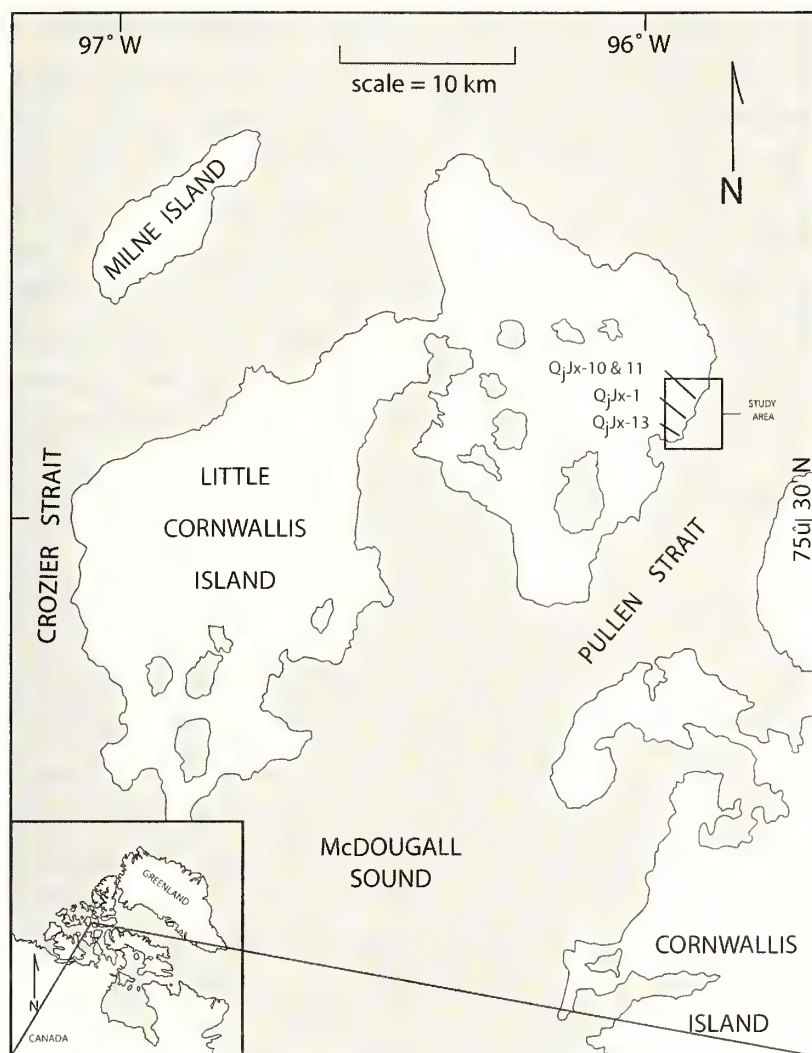
We began implementing our planned strategy (perhaps rather optimistically) a month prior to the 1992 SSHRC adjudication meetings. Toward the end of February, we began advertising our proposed field course in archaeological techniques by sending notices to various Arctic College campuses and selected communities throughout the Northwest Territories, including Resolute Bay (this aspect of the project was funded by a University of Calgary Special Sessions Innovation Fund). On April 1, 1992, we learned that we had been awarded our grant. We contacted the Resolute Bay Settlement Office by telephone shortly thereafter to inform them that our project had sufficient funds to proceed and to inquire about any potential community field school applicants who would be interested in working with us.

One very positive outcome of this initial exchange was a verbal agreement, reached with the Settlement Administrator, to co-submit an application to the Access to Archaeology program (Communications Canada) seeking funds with which to augment community involvement in our project.² Subsequently, we prepared a draft proposal and sent it to the Resolute Bay Hamlet Council for its approval on April 10, 1992. Although the council meeting at which our proposal was to be discussed was canceled, the Settlement Administrator personally contacted council members to obtain their approval. We were informed, in a letter dated April 27, 1992, that our proposal had been approved by a majority of the council, signed by Mayor George Eckalook, and forwarded to the Communications Canada Office in Winnipeg.

Up to this point, our efforts to promote community involvement in our research program appeared to be progressing smoothly. On May 10, 1992, we submitted an application for an archaeological permit to the Prince of Wales Northern Heritage Center in Yellowknife. This is when things began to unravel for us.

During the first week of June, we received our first notice that formal approval of our permit application had been denied by the Resolute Bay Hamlet Council. All other archaeologists planning to do fieldwork in the Resolute Bay area during the summer of 1992, we were informed, had received similar notices. No formal explanation as to why our application was being contested at this late date was forthcoming.

Understandably, this news caused us a great deal of consternation. Our immediate response was to arrange a face-to-face meeting with council members to discuss our project and to determine a possible resolution of their concerns. Toward this end, one of us (LeMoine) flew to Resolute Bay early in the second week of June to make a formal slide presentation to the council and to outline, more fully, our proposed research program. In making this trip, we hoped to identify and respond to the council's specific concerns.



20.1/ Map indicating site locations on Little Cornwallis Island

(Helmer et al. 1993; LeMoine et al. 1993). At this point, we had not altogether abandoned hope of obtaining belated permission to excavate, but we developed a revised research plan that involved detailed surface examination and mapping of the sites, and recording, cataloging, and photographing objects.

In the first week of July 1992, we began fieldwork at Qjlx-10, the largest of the three Late Dorset sites located on the southeastern portion of Little Cornwallis Island (fig. 20.1). Once in the field, we proceeded with our original plan to bring a group of community elders and council members to our camp on Little Cornwallis Island. We hoped that this visit might result in a change in the council's position vis-à-vis permission to excavate (Helmer et al. 1993). Council members, as well as several elders (including Simonie Amarualik, his wife Sarah, and Minnie Allakariallak), arrived at our

We also hoped, of course, to obtain its permission to proceed with our excavation plans.

LeMoine's trip was only partially successful. As a result of her efforts, we learned that council members were generally in favor of specific elements of our proposed program and that they were willing to grant us permission to do surface assessments of the archaeological sites in our study areas (but not to collect). We also learned that they had some practical concerns about the way archaeological research had been conducted in the central High Arctic in the past.

Although we did not obtain permission to go ahead with our planned excavations, we resolved to go into the field at the end of June to pursue as many of our original archaeological research objectives as possible

field camp on July 16.³

We took the elders and council members on an extensive tour of Qjlx-10 to show them the variety of architectural features that are preserved at the site and some of the surface artifacts (still in situ) that had been located up to that point. During the tour, we discussed what we hoped to accomplish at the site through our research. We also invited our guests to provide us with their observations, comments, and queries about the site, the artifacts they had seen, and the surrounding area. At the end of the tour, we asked Simonie Amarualik to recommend an appropriate name for the site. He suggested *tasiarulik*, or "place of many small lakes," which is an apt description of the area surrounding the site.

Before our guests departed, we asked Mayor Eckalook to bring our request for permission to excavate at the Tasiarulik site forward to the council for a third time. We further requested that he inform us (via the Polar Continental Shelf Project office in Resolute with whom we were in direct radio contact), should there be any change in the council's position during the remaining four weeks of our field season. Regrettably, the council did not reverse its decision.

On our return to Resolute at the end of the season, we met with Mayor Eckalook to discuss our plans for future fieldwork on Little Cornwallis Island and to arrange for several public presentations of our work the following Spring. Although noncommittal about the possibilities of fieldwork in 1993, he did offer us some useful suggestions for future community presentations. He specifically requested that we come to the settlement at some point to speak to local schoolchildren about northern prehistory. He also asked that several children be allowed to participate in next year's mid-season visit to the Tasiarulik site. This, he suggested, would give the youngest members of the community a valuable opportunity to learn about the past from their elders.

The Dilemma Facing Archaeology in the North

Thanks to the 1992 field season, we learned that the residents of the Hamlet of Resolute Bay share many concerns that we, as archaeologists, can directly address. For example, several people we met expressed their dismay over the fact that dozens of "southern" scientists come to the Arctic every summer to carry out their research and then return to the south without ever directly consulting with the community—either about their work or their findings. We actually encountered a great deal of resentment over this neglect. Although many of us (southern-based scientists) have felt that we were actively communicating with local communities about our research, obviously we have not been doing this effectively or meaningfully.

We were also confronted with a number of commonly held misconceptions about the discipline of archaeology itself. Many people expressed their dismay at what they perceived to be the wanton destruction of archaeological sites through deep excavation. We were told that "tourists visiting the North want to see old houses, not holes in the ground." The council members who visited our site last summer appeared to be surprised when we explained that most excavations in the Far North—at least at Paleoeskimo sites—are seldom deeper than 5 or 10 centimeters and that excavation units were backfilled upon completion. We also pointed out to them that only a small portion of an archaeological site was ever excavated.

On several occasions, council members asked about the ultimate "fate" of the artifacts recovered during excavations. Some believed that the artifacts were simply taken south and sold. This view, we might add, is closely linked to the perception that the sole motivation that brings archaeologists to the North to dig is profit. Others on the council firmly believed that artifacts, once removed from the North, merely sit "in dusty basements" where no one can see them. In their view, artifacts should not leave the North, and those that have been removed should be returned to the communities where they have been found.

Rightly or wrongly held, these objections are all legitimate concerns. They are also concerns that, in theory, can be directly addressed by archaeologists through ongoing communication, education, and negotiation. Unfortunately, these concerns are perhaps the least of our worries.

Although never directly expressed to us by the hamlet council, there appears, upon long reflection, to be two primary issues involved in our ongoing permit dilemma. The first concerns the perceived relevance of archaeological research to the residents of Resolute Bay themselves. The second, which we feel is inextricably linked to the first, relates to the tangled web of local, territorial, and national politics.

The Relevance of Archaeology

During the past several decades, Western archaeologists have grown increasingly receptive to the view that there are multiple ways of perceiving and interpreting the past, each with its own internal logic and justifiable rationale (e.g., Bielawski 1989; Trigger 1989). For the last ten years, Bielawski (1989) has studied what she calls the "indigenous science" practiced by the Inuit of northern Canada. She has done an admirable job, we think, of articulating the differences between Inuit and Western perceptions of the land and its past.

As professional archaeologists, we have dedicated our careers to studying northern prehistory using the tools of Western science and philosophy. We have striven, over the years, to create rigorous, objective, and, to us, intrinsically fascinating reconstructions of the past. The problem, as Bielawski has made abundantly clear, is that our interpretations, no matter how carefully crafted, have little or no relevance to the people of Resolute Bay. As we have been told by members of the hamlet council, "Our elders already know all that we need to know about the past. What you have to say is not important to us."

In an insightful article, McGhee (1989) has discussed some of the inherent contradictions between Native and Western approaches to the interpretation of the past. McGhee argues that the "two solitudes" in this conflict stand to benefit more from cooperation than from confrontation. He suggests further that the archaeological community, on one hand, and indigenous peoples, on the other, should strive to strike a balance in which both parties would agree to "share" their knowledge and perceptions of the past.

McGhee's position is one that we fully endorse but one that does not seem to appeal to the residents of Resolute Bay. The dilemma confronting us is that, insofar as the Resolute Bay Hamlet Council is concerned, we, as archaeologists, have nothing to offer them that they are interested in having. They do not see the value of alternate (i.e., Euro-Canadian) interpretations of the

archaeological record. Although they may not own the past, they have de facto control over our access to it, at least in the form of newly excavated data. What then is their incentive to negotiate?

Pawns in a Political Chess Game?

One final issue confronting us in our efforts to gain council approval to continue archaeological research is perhaps less a question of differing ideologies than it is of local perceptions of political authority. For the past several years, the indigenous peoples of the Northwest Territories have been engaged in the comprehensive negotiation of land claims issues. These negotiations have recently resulted in Federal recognition of the territory of Nunavut in the Eastern Arctic. Many local land claim issues have also been recently settled, or are very close to being resolved.

Land claim negotiations in the North have not always been easy or amicable. There has been frequent friction and disagreement between the various local, territorial, and federal jurisdictions. In the High Arctic, tensions surrounding land claims issues have been further exacerbated by the ongoing controversy over the "resettlement," by the federal government, of several Inuit families from northern Quebec to Resolute Bay and Grise Fiord in the 1950s. Feelings of betrayal, loss, and neglect continue to run high in both communities of "High Arctic Exiles."

Over the years, the Resolute Bay Hamlet Council appears to have developed the strong impression that it possesses little or no political authority, even within its own local area. In the context of our current permitting problems, the council has long recognized that it has the nominal authority to approve permit applications for archaeological excavations. It remains convinced, however, that the territorial government is not interested in its opinions and is willing to issue permits regardless of local concerns.

Looking at the situation from this perspective, we can suggest that the Resolute Bay Hamlet Council may have rejected all archaeological permits for the 1992

season simply to test the strength of its newly found political authority. The fact that council members who visited our site this summer were both surprised and pleased to find that we had not received permission to dig by the government suggests that this might be so. Perhaps now that the council has successfully demonstrated its political strength in this matter, it will leave the door open for future negotiations. Conversely, this experience may very well encourage the council to become even more restrictive toward scientific research in the North. Only time will tell.

Implications for the Future

It is clear from our experiences in 1992 that the nature of archaeological fieldwork in the North is changing. It is fairly safe to say that our freedom to excavate when and where we please will be, in the future, greatly constrained. Archaeologists will have to rely more and more on the kind of non-interventionist, non-destructive survey and surface evaluation of archaeological sites that we employed at Qjjx-10 in 1992.

For Paleoeskimo specialists working in the High Arctic, this may be less catastrophic than it will be for Neoeskimo specialists. In the High Arctic polar desert environment, Early and Late Paleoeskimo sites are seldom obscured by either soil or dense vegetation. In 1992, for example, we were able, through surface examination alone, to identify more than 760 stone, bone, antler, ivory, and metal tools at the Tasiarulik site (Helmer et al. 1993). Among the different types of artifacts we found were a variety of endblades, bifaces, burins, blades, lamp fragments, copper and iron fragments, harpoon heads, darts, an incised "wand," and a variety of zoomorphic and anthropomorphic figurines, including one of the most spectacular examples of a spirit transformation figure that we have ever seen (fig. 20.2). In addition, we recorded and analyzed more than 3,400 animal bones and bone fragments (Darwent 1993, 1995; Helmer et al. 1993). It is an entirely different story for Neoeskimo specialists, how-

ever, since the sites in which they are interested are often heavily vegetated and contain relatively few visible surface artifacts.

Another change that we may see in the future is an increase in the amount of targeted research. As local communities become more aware of the commercial benefits accruing from the tourism industry, hamlet councils may begin asking archaeologists to cooperate in the excavation and restoration of specific archaeological sites located near their settlements. Perhaps if archaeologists can demonstrate their worth in this way, attitudes toward more "scholarly" archaeological research will change for the better.

Archaeology in the Canadian North is not, in our opinion, a dead issue. There are still research options open for us to pursue, most notably, some of the non-intrusive techniques we used in 1992, as well as more technologically advanced methods, such as synthetic aperture radar,⁴ now under development. To survive as academic researchers, however, we have to become more flexible and innovative in our responses to the changing sociopolitical environment in which we live and work.

Our experience in Resolute Bay has taught us that we must take the initiative to involve the community in our work, to educate them about what we do and why we do it, and to find out from them what they know about the past, what they would like to learn (if anything), and how we can help. These goals can, in part, be achieved by producing general interest articles, museum displays, videos, and other educational tools, but this is only part of the answer. Personal contact is an essential part of this. We have had great success, and a lot of fun, with school talks, including hands-on activities, and with an open-house display in the community at the end of the field season. We have also been lucky to be able to bring members of the community out to our site to see archaeological work in progress. All of these efforts, and more, are necessary to establish and maintain good relations with the people of the North.

Resolution in 1993

When we originally presented this as a paper, we were awaiting a decision from the Hamlet of Resolute Bay on our application for a permit to conduct excavations at the Tasiarulik site in 1993. In this application, we outlined our plans to test the representativeness of the surface information, which we had collected at QJJx-10 in 1992, by excavating a selected sample of specific feature types at the site. We stressed the urgent need to formally assess the statistical validity of conducting surface evaluations of Paleoeskimo sites if our access to such sites was going to continue to be restricted. In early June of 1993, both of us traveled to Resolute Bay to meet with the hamlet council to discuss our application and to give an illustrated presentation to the local school. Council members listened very carefully to our arguments and approved our request for an excavation permit. The formal testing of the surface data collected in 1992 began at the end of June 1993.

Postscript 2001

Much has happened since this chapter was originally prepared. We have completed two fields seasons of excavation (Helmer et al. 1995a, 1995b). Nunavut became a reality, and as a result the process for approving archaeological excavation permits (and a multitude of other heritage-related issues) has changed. In retrospect, the results of the “crisis” have been positive. Our original goals for community outreach have been met, thanks to an ongoing dialog with community members, and they have

been met in what we hope were productive ways. After the 1992 field season, Mayor Ekalook had suggested that children accompany community elders on future site visits so that they could learn firsthand about their past. Accordingly, in 1993 a group of people of all ages visited the site, again at the courtesy of Cominco (Polaris Mines) Limited, which provided a Twin Otter aircraft. We also planned a visit for the 1994 field season but were forced to postpone it initially and then to cancel it because of poor

flying conditions. We visited the community more often (in the spring of 1993 and again in 1994, and a final visit in 1998). We met with the hamlet council and spoke to local school children on each occasion. Although a public slide show in the spring of 1994 attracted little attention, an invitation to the community to see artifacts recovered in 1994 was very popular. Finally, in the spring of 1998, we prepared a photographic display that documented the project in Inuktitut and English. (The artifacts themselves are currently housed in the Prince of Wales Northern Heritage Center in



20.2/ *This Dorset ivory carving of a spirit transformation figure was located on Little Cornwallis Island in 1992, but was not collected until permission was granted in 1993.*

Yellowknife; they will be transferred to a facility in Nunavut when it is built, but it appears unlikely that they will be housed at Resolute Bay.) When we last heard, there were plans to install the photographic display in the village’s new community hall, which was then under construction. Altogether, three students from Resolute Bay worked with us in the field, as

well as one Arctic College student, who received college credit for her work.

From an academic perspective, the “crisis” also had a positive impact. Perhaps most significant was the opportunity to test a new data collection technique in a controlled field situation. As we pointed out to the hamlet council in 1993, the surface material that we had collected in 1992 was impressive, but it was difficult to compare with other collections recovered by excavation. By excavating the site, we generated a sample that could be used to compare the surface sample with the subsurface sample, and to evaluate the utility of this technique for future use. Work on this is ongoing (see Helmer et al. 1994). Even before the analysis was complete, we felt that the surface survey technique we had developed in 1992 was worth continuing and applied it to all areas designated for excavation in the following years. We also tested other noninvasive survey techniques, the most successful of which was a metal detector (Rast 1995). Other members of our field crews have gone on to apply these surface evaluation methods at other sites (Dawson 1997).

Viewed more broadly, our 1992 “crisis” turned out to be a mere ripple. As far as we can tell, there have been no long-term negative effects on northern archaeology. Other researchers have continued to work in the area, although not all of them have received permission to excavate. This factor is probably due more to changes in the overall permitting structure in the

new territorial government than to any positive (or negative) impact of our research. We can hope that our community outreach efforts have had some positive results, but we will probably never be able to tell with any certainty. Both the “crisis” itself and our reaction to it are best seen in a broader context. This involves the political action underway on the eve of the formation of a new Canadian territory, and the widespread recognition among archaeologists that communication with the people in whose communities we do our research is not simply a line in a grant proposal but an important and necessary part of every research project.

Notes

1. This chapter was originally prepared for publication in 1993. We have chosen to let the main text stand as it was then, so it does not reflect important changes in the political situation of what is now Nunavut. Readers who are interested in the archaeological results of the fieldwork can consult: Helmer et al. 1993, 1995a, 1995b; LeMoine et al. 1995; LeMoine and Darwent 1998. A postscript summarizes the current state of affairs.

2. This application was unsuccessful.

3. We are greatly indebted to Tony Keen and Jim Armstrong of Cominco Resource’s Polaris Mine Operation for organizing the Twin Otter flights and other arrangements that brought our visitors into the field.

4. Tim Davies is currently developing this promising technique at the University of Calgary.

Inuit Participation in the Archaeology of Nunavut: A Historical Overview

SUSAN ROWLEY

Today we are all well aware of the negative stereotype of archaeologists that exists in Nunavut. In this chapter, I examine the genesis of this stereotype by exploring the history of Inuit involvement with archaeology. I have divided this history into five time periods: (1) Pre-Contact; (2) European Contact and the Beginnings of Non-Inuit Archaeology (late 1500s to World War II); (3) Alienation (post-World War II); (4) Politicalization and Change (1977-1993); and (5) The Next Step (post-1993). The fourth section contains an extensive discussion on the Igloodik Archaeology Field School, which represents one of several attempts by archaeologists to address Inuit concerns about archaeology.

Pre-Contact

Prior to contact with Europeans, Inuit children grew up in a culture in which history played an active role in their daily lives. Infants were given names that had belonged to deceased relatives. A child's name guided the child through his/her relationships with others. Rather than referring to people by their given names, Inuit used *turslurausiit* (kinship terms). These terms, however, were not those of the child but rather those of the individual for whom the child had been named. Hence, a young girl named for her maternal grandfather would be referred to by her mother as "little father." Children frequently received more than one name and were referred to by the preferred term of the individual by whom they were being addressed. This use of kinship terms kept people's memories alive from generation to generation.



As children grew, they were surrounded by history. Through history, youngsters learned not only about the past but also about their land and their culture. They learned about the location of resources and the fluctuations of fauna. They learned about behaviors that were considered acceptable and those that were considered unacceptable. Learning these things was an important part of growing up, since a child's survival depended upon this knowledge.

Geographical place names reflect Inuit heritage. Many places are named for their physical characteristics, such as *Itillukuluk*, which means "pleasant little land crossing between two bodies of water." Others are named for their resources, such as *Uluksarnat*, "place where there is slate," or *Nirliviktuuq*, "place where there are many Canada geese." Still other locations are named after historic events, such as *Iksivauttaujaq*, "like a chair," on Igloodik Island, which commemorates the history of two brothers, and *Uujaarsiartalik*, "the burial place of Uujaarsiaq." Finally, some places are named after archaeological features, such as *Iglurjuat*, or "place of big houses."¹

Young Inuit had different ways of learning about the past. One was through an understanding of their names and their relationships to others in the community. Another was through learning the local geography. Still another way was through oral history. In 1862, the Inuit informant Ebierbing (also known as Joe) told Charles Francis Hall how Inuit use oral history to transfer knowledge from generation to generation:

When our baby boy gets old enough, we tell him all about you, and about all these kodlunas who brought brick, iron, and coal to where you have been, and of the kodlunas who built a ship on Kodlunarn Island [Sir Martin Frobisher]. When boy gets to be an old Inuit he will tell it to their Innuits, and so all Innuits will know what we now know. (Hall 1864, vol. 2:171)

While oral traditions were the most common way of imparting history, Inuit also used another unusual approach—an indigenous, small-scale archaeology. Many archaeological sites in the Arctic are highly visible. While waiting at Inuit land camps for the ice to break up in early summer, the elders and children often dug into nearby old dwellings for educational purposes or to collect raw materials for tool and art manufacture. The elders used the artifacts they found to instruct youngsters about past lifeways. Although most of these artifacts were left at the site, some were curated and taken from camp to camp. In certain cases, these artifacts were used as amulets.

I was assured that broken spear-heads, and other equally cumbersome pendants, worn around the necks of young girls, were spells for the preservation of their chastity; while the same ornaments caused married women to be prolific. (Lyon 1824:368)

Artifacts that had been used by great hunters were sometimes kept and passed on to succeeding generations. Sometimes harpoon heads were obtained from elders who were no longer able to hunt in the belief that the elders “luck” would be transferred or that special implements could acquire properties that actually attracted seals (Mathiassen 1928:150). Inuit held the land in great respect and this respect included

archaeological sites. Occasionally, if a site was the location of a starvation camp or curse, it was feared and avoided. Respect was also shown to bones that were found lying on the land.

When we came across old bones on the ground, some might even be so old that part of it would be covered with earth, we would lift the bones up and lay them down again with the other side on the ground. This was so that the bone could rest by placing the side that was on the ground facing away from the ground. There were all kinds of things observed at the time when the taboos were strictly adhered to for the purpose of appeasing [the unknown]. (Noah Piugattuk, Igoolik Oral History Project²)

Years ago it was said that the bones get *tuningurtaqtut* (tired of being in the [same] position for a long time). So it was said that the bones should be flipped or repositioned in order to let it rest from the same position. (Suzanne Niviattian, Igoolik Oral History Project³)

During the initial period, then, Inuit controlled their own past and its interpretation. They had traditions concerning their origins, their history, and the recent past. These traditions were transmitted from one generation to the next through people’s names, local geography, oral history, and archaeological remains.

European Contact and the Beginnings of Non-Inuit Archaeology (late 1500s–WWII)

During the second period, the transmission of Inuit history to the next generation changed very little, but with the arrival of Europeans, outsiders began to sequester for themselves control over Inuit history. As early as 1767, Europeans were inquiring about the origins of Inuit culture (Crantz 1767).⁴ As European interest in the Arctic grew and whalers, traders, and missionaries moved into the region, more and more ethnographic artifacts found their way to the museums of Europe and North America. In the early 1900s, Franz Boas asked several whaling captains, including George Comer, to collect artifacts for the American Museum of Natural History in New York during their voyages. In a letter dated May 31, 1906, Boas’s colleague Clark Wissler wrote to Comer, saying that:

"we are especially interested in archaeological material, or such material as may be found in old deserted villages and burial grounds. We should like all of this material you can get, and you should bear in mind that the more ancient those village sites are the more valuable the specimens obtained from them will be."⁵

Wissler's last comment certainly provided Comer with an incentive to dig at archaeological sites. Prior to this, Comer had collected skeletal material for the museum from the west coast of Hudson Bay. In June 1905, when Comer's whale boat was smashed by a whale, however, the Inuit contended that the spirits of those individuals whose skulls had been collected had entered the whale and punished Comer. They warned Comer to stop collecting skulls to ensure his continued safety and success in the whale hunt. Comer (1906:483) wrote: "It is needless to say that I did not tamper with skulls anymore." Whalers like Comer and traders like George Cleveland (Eber 1989:125-127), who collected skeletal material at the behest of anthropologists and archaeologists as well as of members of the later Fifth Thule Expedition, are primarily responsible for the view, commonly held among the Inuit of Nunavut, that archaeologists are grave robbers.

When the Inuit realized that old artifacts were of interest to outsiders, they began to assemble collections, which they offered for trade. Among the many collections put together in this way were:

The Dutilly collection, now housed at the Canadian Museum of Civilization in Ottawa, it came from a number of sites along the west coast of Hudson Bay and Melville Peninsula. The artifacts were collected by Inuit who brought the material to Roman Catholic missionaries in the 1920s and 1930s. Later, these collections were acquired by Father Dutilly as he traveled along the coast on the ship *Therese*.

The Burwash collection, also housed at the Canadian Museum of Civilization. On a visit to Cape Dorset in 1924, L. T. Burwash purchased a small archaeological collection from Inuit and obtained a larger collection from the local Hudson's Bay Company post.

The Bazin collection, which was obtained by G. W. Rowley for the Museum of Archaeology and Anthropology in Cambridge, England. During a visit to Igloodik in 1937, Rowley acquired the small collection from the local priest Father Bazin. The collection had come from a nearby walrus hunting camp called Avvajja (Abverdjar).

In many cases, these collections played important roles in the development of arctic archaeology. For example, after examining the Burwash collection, Diamond Jenness (1925) was able to postulate that a culture, which he provisionally called the Cape Dorset culture, had existed in the Canadian Arctic that was older than the Thule culture. In another case, G. W. Rowley, inspired by the Bazin collection, returned to the Arctic to excavate at Avvajja, which turned out to be the first recognized pure Dorset site (Rowley 1940, 1996, this volume).

Between 1921 and 1924, the Danish Fifth Thule Expedition swept across the Canadian Arctic marking the beginning of systematic archaeology in Nunavut. Although Therkel Mathiasen was the archaeologist on the expedition, other expedition members, including Rasmussen, Birket-Smith, and Freuchen, also conducted excavations and assembled large archaeological and ethnographic collections. Subsequently, this material was exported to Denmark, and only a few token specimens have been returned to the Canadian Museum of Civilization.

Following the Fifth Thule Expedition and other foreign expeditions into the Canadian Arctic, legislative steps were taken to ensure that the Canadian government was properly informed of all exploration and scientific expeditions. In 1926, the Ordinance Respecting Scientists and Explorers, the precursor of today's Northwest Territories' Scientific Research Licence, was passed; this ordinance required permits for all scientists and explorers visiting the Northwest Territories. This was followed in June 1930 by the passage of the Eskimo Ruins Ordinance, which prohibited excavations in the Northwest Territories without a license

and made it illegal to transport artifacts out of the country and out of the Northwest Territories without the permission of the Commissioner of the Northwest Territories. The ordinance imposed a \$1000 fine or six months of imprisonment for violations. While this legislation was designed to ensure that foreign expeditions did not remove cultural property from Canada, it also made it illegal for Inuit to dig in the places occupied by their own ancestors. Inuit were largely unaware of the existence of this legislation and it was never enforced by the RCMP.

With the arrival of European archaeologists in the 1920s, Inuit were occasionally employed as field assistants and their expertise was sought for identifying animal bones. At the same time, archaeologists tried to discourage Inuit from digging in sites. They were concerned that the sites were being dug solely to reclaim artifacts for sale and that the archaeological context of the artifacts was being destroyed (H. B. Collins, personal communication; G. W. Rowley, personal communication).

During the second period, Inuit continued to control their past and its interpretation within their own culture but not the interpretation and presentation of their history to the rest of the world. Those who interpreted Inuit culture to the outside world had often spent several years in the Arctic living with Inuit and learning to speak Inuktitut. However, the great archaeological debates of this time—first between Mathiassen and Birket-Smith and then between Mathiassen and Jenness—made little use of Inuit interpretations of their past and origins. Inuit were beginning to be alienated from their past.

Alienation (Post-World War II)

Following World War II, non-Inuit control over arctic archaeology and the interpretation of Inuit history was completed. Three factors—improvements in transportation, a movement toward a “scientific” archaeology, and education in the south—were primarily responsible for this trend.

The construction of airfields in Nunavut at Iqaluit and Coral Harbour (on Southampton Island) during World War II and a third airfield at Resolute Bay shortly after the war allowed archaeologists, for the first time, to arrive in and depart from Nunavut during the same year. It is no accident that the first archaeological projects undertaken after the war were at Iqaluit, Resolute Bay, and Southampton Island (Collins 1950, 1951a, 1956a). The transportation infrastructure was strengthened with the construction of the Distant Early Warning (DEW) sites in the late 1950s. In 1958, the Canadian Government established the Polar Continental Shelf Project (PCSP), which provided free logistical support to scientists working in remote areas of the Arctic. At about the same time, large soft airplane tires were developed, allowing the versatile Twin Otter plane to take off and land almost anywhere (Anonymous 1974). These developments made it possible for scientists and archaeologists to come and go during the same season and to work in areas far removed from Inuit camps and communities. They enabled archaeologists to become the “Cowboys of Science” (Robert McGhee quoting Gero [1985:983]). Finally, they ensured that most archaeologists would rarely have contact with Inuit and would, instead, bring their field crews from the south.⁶

In the period after World War II, there was a complete turnover in the roster of archaeologists who conducted research in Nunavut. Before the war, those working in the region were trained in Europe; after the war, they all came from North America. This change resulted in a move away from the more “humanist” traditions of European archaeology and toward the more “scientific” approaches of North American archaeology. The new arctic archaeologists readily adopted cultural ecology and the “New Archeology” and viewed cultural ecology as especially well suited for explaining cultural adaptations to the harsh Arctic environment. This more scientific approach effectively left little or no role for Inuit and their interpretations of the past.

Finally, there was a change in educational programs during this period. Canada began to establish schools in the north. These schools taught southern knowledge and southern values rather than northern indigenous knowledge, which was not valued and was frequently regarded as knowledge that was rapidly disappearing and largely irrelevant. When students were taught about their own past, their teachers frequently relied on books written by archaeologists and anthropologists rather than consulting with local Elders.

These factors meant that, by the mid-1970s, Inuit prehistory had been completely sequestered from the Inuit by southern archaeologists. Not only were archaeologists interpreting Inuit history for the outside world but they were also telling Inuit what to believe about their past. This divorce between archaeology and Inuit knowledge was exacerbated by the fact that most archaeologists only visited the Arctic in the summer and rarely communicated with Inuit.

Politicization and Change (1977–1993)

A period of political awakening for the Inuit of Canada followed in the 1970s and 1980s. In 1971, the U.S. Congress passed the Alaska Native Claims Settlement Act (ANCSA), which set aside land for Alaska's native peoples and provided a cash settlement in exchange for other lands. Quick to learn from the American experience, the Inuit Tapirisat of Canada (1977) submitted their land claim *Nunavut: Our Land* to the federal government in 1977.

As Inuit became more politically active, their voices were heard more frequently outside the north. People became aware of not only how Inuit felt about their land but also how they felt about the way they and the land were treated by outsiders. Southern archaeologists learned—to their consternation—that many Inuit held strong negative attitudes toward archaeologists and that they perceived archaeologists as people who stole Inuit cultural property and earned fame and fortune from this thievery.

While most archaeologists were appalled by this characterization, they also recognized that the stereotype had some validity. Several archaeologists reacted by developing programs designed to alter these perceptions.

One of these was the Thule Archaeology Conservation Project, initiated by Allen McCartney and co-sponsored by the Archaeological Survey of Canada. In the 1970s, Inuit artists began creating large sculptures out of whale bone. With no ready source of modern bone, they turned to archaeological sites. At first, the artists removed whale bone from the surfaces of sites. When this source was depleted, they began excavating old dwellings to remove the whale bone that served as structural supports. The Thule Archaeology Conservation Project was a response to the heavy damage that resulted at archaeological sites. The project aimed to create an inventory of sites that contained surface whalebone. After the sites and the bones had been mapped, the surface bones were stockpiled for the use of artists (McCartney 1979). This project acknowledged the need of Inuit for this resource, and it came up with an innovative approach that served the interests of both Inuit carvers and archaeologists.

Another project designed to alter perceptions was the establishment of the Northern Heritage Society Field School. Founded in 1979 by Ellen Bielawski and Sally Cole, the society sought to provide an environment for northern youth where they could be exposed to the sciences, including archaeology, in a field setting. The field school operated from 1979 to 1986, when funding became more difficult to secure and when other organizations began addressing the same concerns. In 1987 and 1988, the society supported several students at archaeological sites in the Arctic and sub-Arctic. It also maintained a database of northerners who had attended the field school and were available to work as trained field assistants for scientists engaged in northern research.⁷

The Prince of Wales Northern Heritage Centre (PWNHC), which is responsible for all archaeology



21.1/ *Students learn to knap obsidian from George Qualut*

conducted in the Northwest Territories, initiated two programs to increase Inuit awareness of and involvement in archaeology. In 1981, the PWNHC developed a traveling exhibit on archaeology, which consisted of several display cases of replicated artifacts and illustrated the differences between the Dorset and Thule cultures as understood by archaeologists.⁸ This exhibit met with varying degrees of success: while many people were interested in it, most communities wanted to see real artifacts and displays that were relevant to their particular region.⁹ From 1983 to 1987, the PWNHC also sponsored an archaeological training program for northerners. Directed by Chuck Arnold at the archaeological site of Gupaaq in the Mackenzie Delta, this program focused on training students in field techniques.¹⁰

In 1977, the PWNHC and the CMC instituted a new permitting process for archaeological work in the Northwest Territories that gave local communities more input. Prior to that time, applicants had submitted their requests to a committee of archaeologists from the Archaeological Survey of Canada, which consulted with officials in the Northwest Territories and later with the PWNHC. Their decisions were based on the applicant's qualifications with no input from Inuit. Beginning in 1977,

applicants were required to request permission from officials in communities nearest their research sites. Community permission became a required component of the application process but, because no legislation had been changed, the committee retained the power to grant permits even if the community refused permission.

The archaeological component of this program developed from work undertaken by Doug Stenton at Qarmaarviit for his master's thesis. Stenton employed Iqaluit residents to work

with him on these excavations. Later, he taught at the Nunatta campus of Arctic College. When the college's archaeology program became an integral part of the Environmental Technology Program in 1987, it offered two courses—a field program located at Peterhead Inlet and a lab methods course taught at the college campus in Iqaluit.

Another project was developed in the late 1980s by John Jamieson, the principal of the school at Sani-kiluaq on the Belcher Islands. Fascinated by archaeology and especially by experimental archaeology, Jamieson organized a 1988 workshop for teachers from the Baffin Divisional Board of Education to learn about arctic archaeology. At this workshop, Inuit elders demonstrated skin working techniques and flint knappers from the south demonstrated flint knapping (fig. 21.1). Teachers were able to make casts of archaeological artifacts from the Arctic using molds provided by the Canadian Museum of Civilization.

The last project that I describe here as an example of a program designed to initiate a dialog between Inuit and archaeologists is the Igloodik Archaeology Field School.¹¹ This program began in 1990 as a joint effort between Carolyn MacDonald, an Igloodik teacher,

and myself. In 1987, I started a project on the pre-history of the Igloodik region and Carolyn assisted me in the field. She watched as every year I tried in different ways to involve the youth and the elders of the community in my projects. To some degree we agreed that I was successful. The youth were interested and came away from the work with an increased pride in their heritage. However, there was always a conflict between archaeological fieldwork as a learning experience and as a nine-to-five job. This conflict existed not just in my mind but was also felt by the youth.

One afternoon while conducting a survey, we stumbled upon a partially bulldozed late Dorset site (Qalirusiujak, NiHf-45). Carolyn suggested that the site was an ideal setting for an archaeological field school run under the auspices of the local school. We consulted with the mayor, the hamlet administrator, and the school principal, all of whom were favorably disposed to the field school concept. The field program began in 1990 and has continued every year since (fig. 21.2).

Each year we enroll a total of eight to eleven students. The students can take the course for three years, receiving credits at grades 10, 11, and 12. During the first year, the students' credits were registered under the NWT Department of Education's Special Projects subject area. In 1991, following our request, the Department of Education recognized archaeology as a separate subject area, and the credits the students now receive are designated in the field of archaeology.¹²

Curriculum development is always a difficult task. What are the aims and objectives of the program and how best can these be realized? In our case, the task was twice as difficult as we tried both to involve the local community and to provide a course that resolves some issues about archaeology and southern science for the students. We have tried new approaches every year, keeping what works and throwing out what has not. After three years, we believe we have created a basic program that functions well both for the community and for us as educators and archaeologists.

The Igloodik Archaeology Field School has a number of objectives that deal with community concerns not only about archaeology and control over the past but also about education and the problems faced by today's youth. Among these goals are:

1. To introduce students to the study of the past both through archaeology and oral history. We aim to give them an understanding of the time depth and remarkable achievements of their culture. We, the outsiders, encourage the students' pride in their culture by demonstrating our respect for the achievements of their ancestors and the knowledge of today's elders.
2. To build students' self confidence levels



21.2/ Students excavating and mapping at Arnaqaaksaat (NiHf-4)

so they can succeed, we give them the ability to alter both their grades and the number of credits they receive for the course. We also treat them as adults by making our expectations of them very clear and vice versa. For many of them, this course is similar to a first job and we want this experience to be as positive as possible. We use many different teaching methods and assessment techniques to discover the strengths of each student; these include team work, individual assignments, repetitive tasks, and creative writing. In conjunction with building self-confidence is responsibility; students who are enrolled in the course for a second or third years are often placed in charge of the excavation for periods of time. As their self-confidence grows, so too do their responsibilities and our expectations.

3. To demonstrate how southern scientists go about their research. We outline the scientific method. For some students, this is their introduction to this concept; others have taken biology and chemistry and have an understanding of this set of procedures. We start with a series of questions and ask how these questions can be answered. In terms of scientific experiments, we ask what the purpose, method, equipment, observations, results, and conclusions are. For the Inuit, this approach is similar to their own methods of interpreting their universe.

4. To provide students with a forum to practice the skills that they have obtained in school. This includes the use of Inuktitut syllabics for artifact exhibits, art for exhibit design, illustration, and mapping, and English for data recording and journal entries. We also put to practical use the abstract skills the students learn at school in other disciplines, such as mathematics and biology. For example, they apply x, y, and z coordinates for mapping artifacts they excavate; they use the Pythagorean theorem for creating grids, so that they excavate in equal meter square units; they incorporate triangulation for calculating the height above sea level, which provides a relative date for the site; and they use faunal analysis to understand the lives of people in the past.

5. To provide students with skills they can use in the future but that they do not often learn at school. These include using survey equipment, developing negatives and printing photographs, and reading maps.

6. We offer students the opportunity to learn about past lifeways—how to knap flint

and make ground slate tools. The students find that the experimental archaeology component of the course increases their understanding of the skills people had in the past.

7. We try to make the course work relevant to students. Lectures focus on questions about who owns the past and discussions of the Nunavut land claim agreement. We discuss the cultural heritage clauses of the agreement and how these may impact the students and archaeology.

8. The course provides Inuit youth with training for future employment. Under the Nunavut land claim agreement, there is a potential for new jobs with the Inuit Heritage Trust and a preferential hiring provision for qualified Inuit. In addition, there are opportunities in tourism, a major growth industry in the north; Igloolik is seen as a potential locale for cultural tourism where history (both archaeological and oral) would be an important component of the tourism experience.

9. We try to involve the community at all levels. As much as possible, we use community elders to inform our understanding of the past. Toward this end, we invite elders to visit the excavations and help us to interpret our finds and the site. We invite the community to visit the site, and we develop an exhibit for the community. In this way, we involve the community not only through the enrollment of their sons and daughters in the program but also through their participation as experts and viewers of the exhibit.

10. For the larger community, the exhibit that we mount each year is the most important aspect of the program. The students choose the artifacts and the format of the exhibit (fig. 21.3). They prepare all publicity and exhibit text (frequently calling on elders and parents for information and assistance with Inuktitut). They have also organized a contest, in which each visitor to the exhibit receives a paper harpoon head entry form and is asked to guess the number of artifacts that were uncovered that season. The winner receives a cake made by the students and decorated as an archaeological site.

How successful has the Igloolik Archaeology Field School program been at integrating the community and archaeology? Although community support and integration are always difficult to evaluate, we have several indicators of our success. These include the continued support of the local education society for

our grant and permit applications; the continued approval of our permit applications by the hamlet council; the number and quality of students who sign up for the course; the willingness of elders to assist us; and the large number of people who visit the exhibition we mount each year.

The community wants a permanent archaeological exhibit; during the next few years, students in the course will work on developing and producing an exhibit. We also plan to reconstruct a Thule winter house we began excavating in 1992. The elders we have talked with want to see this reconstruction go ahead and hope to use it both as a teaching tool and as a place where they can meet and reminisce.

Throughout the period, southern-trained archaeologists have continued to interpret Inuit history both for the wider public and for the Inuit. They also began to realize that Inuit not only needed to but also must play a greater role in the discovery of this past. Most of these efforts have focused on introducing Inuit to how archaeologists operate in the field and not on the interpretation of the past.

The Next Step (Post-1993)

In 1993, the Nunavut land claim agreement known as the Agreement between the Inuit of the Nunavut Settlement Area and Her Majesty the Queen was ratified and signed into law. Article 33 of the agreement provides the framework for the future development and growth of archaeology. The Inuit Heritage Trust was established in 1994 under the provisions of the agreement to oversee archaeology in Nunavut. Following the establishment of the Inuit Heritage Trust, all archaeological permits¹³ are reviewed by the Trust, which has the right to reject permit applications. Among the justifiable reasons for refusing archaeological permits cited in Section 33.5.6 are inadequate efforts to secure Inuit participation and benefits or inadequate performance of commitments to provide such participation and benefits under permits issued at an earlier date, or the disturbance of a site of Inuit



21.3/ Krista Apak puts the final touches on a display explaining cast making before the exhibition opens.

religious or spiritual significance, as such significance is defined by the Trust in consultation with the Designated Agency [PWNHC or the CMC] (Tungavik 1993:227).

In February 1994, Inuit from across the Canadian Arctic met to discuss their feelings toward archaeology and the future of archaeology in the north. Common concerns expressed throughout the meeting included archaeologists' lack of respect for the land, for Inuit elders, and for Inuit remains; the sense of sorrow, anger, and frustration at the alienation of Inuit cultural heritage through the removal of artifacts from Nunavut to outside repositories; the need for local museums; and the need for archaeological reports to be made available to the communities concerned. Despite a deep distrust of archaeologists among many Inuit, the elders (with the exception of the elders from Labrador) and the youth expressed the belief that there is a role for archaeology in the interpretation and understanding of Inuit land use and occupation through time. At the same time, they felt that archaeology needed to be conducted in a proper manner. At the end of the

meeting, the participants compiled a series of guidelines for archaeologists, which are incorporated into the final report of the meeting (Bennett 1994). This report should be required reading for anyone wishing to conduct fieldwork in Nunavut.

In conclusion, for years Inuit have watched scientists from all disciplines removing material from the north with little or no return of information. In the case of archaeology, this loss has been particularly painful because Inuit have seen their heritage physically being taken away. This pain has been exacerbated as a result of the disappearance of other parts of Inuit heritage, such as the loss of regional dialects and clothing styles and, most important, the passing away of elders who are the major repositories of Inuit cultural knowledge.

While carrying on excavations, archaeologists have a tendency to insist that Inuit have no right to dig in the sites nor to maintain possession of any artifacts, despite the fact that these sites are on Inuit land, that they were occupied by Inuit ancestors, and that the elders today use the evidence that exists for the presence of a pre-contact indigenous peoples as a didactic tool to inform youth about the past.

Inuit want to participate in this interpretation and to formulate their own interpretations. For years now we have been required to receive community approval for our research. We frequently send copies of our reports to the communities and sometimes hire locals to assist in fieldwork. These practices are all well and good but they do little to alter Inuit impressions of archaeologists. Our reports are in English and use archaeological jargon. They are usually sent to the community council and they tend to disappear. It has been rare for archaeologists to involve communities successfully in research. (This is often not the fault of the archaeologists as most archaeological projects take place many miles from any community.)

Since 1977, progress has been made toward forging a dialog between Inuit and archaeologists. We now have to look toward the next step along this

path. Control over the past is held not by those who excavate at archaeological sites but by those who ask the questions and interpret the results. Archaeologists and Inuit share a common desire to learn about Inuit history. The next step should involve a dramatic change in archaeology with Inuit and archaeologists eventually working together to formulate innovative research programs and gain a richer understanding of Inuit history.

Acknowledgments

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I am indebted to Susan Baer at the NWT law library for tracking down the early ordinances that pertain to explorers and archaeologists. I would also like to thank Ellen Bielawski, Margaret Bertulli, and Doug Stenton for assisting me with the details of projects with which they were involved.

Notes

1. These place names are from northern Foxe Basin. The spellings and translations are taken from SINT 1993.

2. Igloolik Oral History Project, Interview IE148. Interview by John MacDonald and translation by Louis Tapardjuk.

3. Igloolik Oral History Project, Interview IE149. Interview by John MacDonald and translation by Louis Tapardjuk.

4. David Cranz (1767) hypothesized that Inuit culture had originated in Northeast Asia, and this concept was later developed by Clements Mark-

ham (1865).

5. Letter dated May 31, 1906, from Clark Wissler to George Comer. Archives, American Museum of Natural History, New York.

6. There were, of course, exceptions. The most notable of these was Moreau Maxwell's work on south Baffin Island.

7. Deborah Webster, a graduate of the Northern Heritage Society Field School, graduated from Carleton University in 1991 with a degree in anthropology. She now works as an archaeologist and northern expert for Parks Canada in Yellowknife.

8. The text for this exhibit was prepared by Ellen Bielawski.

9. In 1979, Bill Fitzhugh and others at the Smith-

sonian Institution in Washington, D.C., organized a workshop on the organization of community museums. This workshop was attended by people from Labrador and Alaska.

10. At the same time, Makivik Corporation and Avataq in northern Quebec were running archaeological field schools that trained local youth.

11. In the early 1990s, both Stephen Loring and Susan Kaplan organized community-based projects in Nain, Labrador.

12. Anyone wishing to start a similar program will be able to use this course designation.

13. There is a provision for an exception in the case of sites that require immediate salvaging.

The Last Known Traditional Inuit Trial on Southwest Baffin Island

NORMAN HALLENDY

In the summer of 1991, while gathering material from Inuit elders on places of power and objects of veneration in the Canadian Arctic, I learned of the existence of a traditional system of justice that once prevailed on southwest Baffin Island. While the place where justice was exercised remains as a visible artifact on the landscape, the traditional way of exercising justice vanished after the arrival of the *qallunaat* (white men).

I was taken from Cape Dorset by open canoe to a circle of large upright stones, which an elder explained was where the Great Council met, "It was like your Parliament."

He called the place and the circle Akitsirqavik. I was told I was the first *qallunaaq* to be brought to this place of power, and was asked not to reveal its location.

This structure, unlike any other I have seen in the Arctic, is constructed of massive stones, some weighing up to a ton, standing on end and arranged in a near-perfect circle. The largest stone measures 7 feet 6.5 inches high by 1 foot 3.5 inches wide by 1 foot 6.5 inches thick. It is opposite the second largest stone, which measures 5 feet 6 inches high by 1 foot 6 inches wide by 1 foot 6 inches thick.

After we returned to Cape Dorset, a second elder, learning of my introduction to Akitsirqavik, revealed another undocumented aspect of the Inuit justice system, a meeting of the Great Council sixty-seven years ago to hear a charge of murder. This is thought to have been the last traditional Inuit trial on southwest Baffin Island.



Normally, the trial would have been held in the stone circle, but events prompted a change in location. The killing of one man by another on a hunting trip had caused a great deal of tension between the families of the victim and the accused. In addition, two members of the council were related to the victim. So, in August 1924, there was an unusual urgency about reaching a verdict. However, a Hudson's Bay Company supply ship was expected any day at Parketuk, about 9 kilometers northeast of Cape Dorset, so the members of the council decided to meet there.

The revelations about the circle of large upright stones and the existence of a formal justice system were given to me just before my departure from Cape Dorset in 1990. Bad weather had frustrated all my attempts to reach another place of power about which I had heard a great deal, Inuksuglait, ("the place of many, many *inuksuit*" [stone constructions]). This remarkable site on the southwest coast of Baffin Island (64°33' north latitude, 70°11' west longitude) has some 200 stone constructions concentrated in a small area

of about 1.3 hectares. Commonly known as Enuks Point, it is a place of great significance to the Inuit of southwest Baffin.

In 1990, I had made what I thought was my final attempt to reach Inuksugalait with an experienced young hunter. Once again, however, we were kept at Cape Dorset by a succession of spring storms.

Just before returning to the South, I had tea and bannock with Pauta and Pitaloosie Saila.¹ Sensing my disappointment, Pauta hinted that if I could return the next summer he might take me to Inuksugalait. Then, after thinking for a moment he added, "And maybe I will be able to show you something even more important." Slowly and carefully he went on to describe a place where there is a stone structure unlike any other, a place of great power and significance. Seeing that I was fascinated by this revelation, Pauta's wife, Pitaloosie, made a detailed drawing of the site for me. Her skilled hand revealed a great circle of upright stones unlike anything I had seen in the Arctic.

Knowing the importance of names to the Inuit, I asked, "What is the circle called?" Pauta had to search far back in his memory, but was able to recall the name which he had learned from his mother. It is called Akitsiraqvik.

Pauta spoke about the place and what had happened there, and I was careful not to interrupt or to introduce notions of my own. He described the place as a kind of Parliament where judges, powerful men like high priests, sat in judgment of the most serious matters.

I returned to Cape Dorset in the summer of 1991 and true to his word Pauta, along with Pitaloosie and other members of his family, took me to Inuksugalait and then to Akitsiraqvik (fig. 22.1). Later, based on the photographic evidence that I provided, both W. E. Taylor and C. Arnold confirmed they knew of no similar site in Arctic Canada.

Upon my return to Cape Dorset from the two great sites, I was informed that Osuitok Ipeelee,² my old mentor, wished to speak to me. So Osuitok, my dear

friend Annie Manning, and I gathered together for a hearty meal and an evening's conversation. Osuitok began by asking me what I had seen during my trip. I admitted I had been taken to Akitsiraqvik. "I know," he said. "Now I have something else to tell you about the old way of justice."

What follows are my notes about Osuitok's recollections of the last Inuit trial on southwest Baffin Island before the arrival of the *qallunaat*'s system of justice.

The Trial

On a calm, clear day in August 1924, a bullet extinguished a hunter's life in an instant. He lies buried just behind the hill from where I write these notes. The fate of the victim's hunting companion who fired that fatal bullet was decided at Parketuk, about 9 kilometers northeast of Cape Dorset.³

At the time, there were at least fifty camps along the coast from Nuvujuak, at the northern tip of the Baffin Peninsula, to Markham Bay, some 260 nautical miles to the south. Some camps had as few as two families, while others had five or more. It was not unusual for camps to grow or shrink in size with the seasonal availability of food.

Each camp had its camp boss⁴ whose leadership was based on demonstrated ability. He was kept informed of everything going on in the camp and in the surrounding area. He had the final word about anything that really mattered, including where and when to go hunting, the division of food, marriages, who could join or leave the camp, and the nature of tasks and any punishments. Lines of authority and indications of respect were clear from the servants or camp slaves⁵ all the way up to the camp boss.

Certain camp bosses and shamans achieved a higher status than other leaders because they were acknowledged to be the best thinkers, speakers, and achievers in the region. They were the *tapananitiit*, "the powerful ones," who merited the highest respect. The *tapananitiit* were also known as the *issumaliuqtiit*, "the wise men." Within this elite group were the *pimariit*,



22.1/ *The traditional trial arena, Akitsiraqvik, on Baffin Island*

“those who could speak powerfully.” They could choose words with great skill and arrange them in amazing ways. Collectively these men who formed a power elite exerted a great influence throughout southwest Baffin Island in traditional times.

Although the *tapananitiit* lived in various camps throughout the region, there were occasions when such matters as murder, pestilence, impending starvation, and difficulties associated with the arrival of the *qallunaat* brought them together for thoughtful discussion and considered action.

The Great Council met in various locations as required by events and prevailing conditions. One place, however, was favored above all others as an enduring symbol of the council’s power. Located in a remote region of southwest Baffin Island known as Qaumajuq (where the land is in brightness), this place was Akitsiraqvik, a large circle of upright stones with one very tall stone seeming to face the structure into the prevailing wind.

The name Akitsiraqvik is so old that its exact meaning has been forgotten, but it suggests “to strike out, to render justice.” In this case and others, the traditional name of an object in a place and the name of that place are one and the same.

And so in Akitsiraqvik was a court where the council sat, questioned the accused and witnesses, heard

confessions, listened to pleas, resolved conflicts, and decided punishments. Unlike any court we know today, it was also the center of a place where celebrations, games, and feasts occurred at various times of the year.

In 1924, the council gathered at Parketuk to be close to the place where the great *umiaq*⁶ (supply ship), the S.S. *Nascopie*, would arrive with tea, tobacco, needles, beads, pots, and all kinds of other useful things.

But the council was also to render judgment on a man, *L*,⁷ who claimed that his shooting of his hunting companion, *O*, was an accident. The unfortunate event might have been endured with quiet resignation except for two factors. First, the victim’s family argued that he had been murdered; as a result, there was great tension between them and the family of the accused. Second, both the victim’s natural father and his acquired father (he had been adopted while a child) insisted that the accused be summoned to account before the council. It happened that these two men were powerful members of the council. Understandably, there was a general feeling that the accused would be found guilty and put to death.

Some council members had been taken to the place where the victim had fallen. They had examined the area where the accused had taken his position to shoot

birds; they had considered the weather, the time of day, and the myriad of details familiar to hunters. Then they met to reach a decision.⁸

Only the accused and his parents were permitted to appear before the council. The accused gave his testimony and answered questions. Having been required to stand throughout the proceeding, he became very tired and lost his balance, but recovered. Then, resigned to dying, he said, "If you decide to kill me, take me away from this place, and shoot me where I will bleed to death slowly. And if that punishment doesn't satisfy you then take my child and do the same."

The council remained silent for a long time. Then Osuitok's father and *P*,⁹ a powerful camp boss, whispered back and forth. Finally *P* exclaimed, "Whoever kills this man removes my will to live!"

His penetrating words struck at the thoughts of everyone on the council, and they decided to spare the life of the accused. However, there was a condition: should the accused ever be involved in another person's death in any way, at any time, his own death would follow swiftly.

The decision prescribed a standard of conduct to be followed for the rest of the man's life. He would be vulnerable whether he was in the presence of friends or strangers, yet to live apart was inconceivable. His fate became known throughout southwest Baffin. To him it meant *inuugiaqarnirama*, which means "my time to die is not yet come, my life is fated to continue."

And so *L*'s life continued without incident. The timeless expression *ajurnarmat*, which means "it cannot be otherwise" had a particular meaning for *L* until the day his earthly journeys came to a natural end.

With the arrival of the great *umiaq* from the south came many desirable and wonderful things. There also came different beliefs and practices, which altered the traditional way of life and erased, at least in this part of the Arctic, the Inuit way of dispensing justice.

Discussion

The defendant did not appear to seek to address the main issue of whether the killing was intentional or accidental. In addressing the council, he seemed to take for granted the fact that he would be convicted. Further, he eschewed making a speech to mitigate his punishment. He appeared to encourage an increase rather than a decrease in the punishment meted out, advocating a slow and painful death over a swift and painless one. Thus, the defendant, at least on the surface, turned his back on the classic advocate's approach of denying responsibility, and if that fails, seeking the least possible sanction.

But this accused must be taken to have known the culture from which he came, and his lack of legal training served him well because he was, nevertheless, able to persuade the key member of the council not to favor the death penalty. Obviously, the accused used the technique of persuasion most suited to his situation in that particular culture. That it would have won no prizes in a southern Canadian law school exercise in trial practice is dwarfed to insignificance by its success in Baffin Island where his life—rather than his formal legal skill—was literally on the line.

An aboriginal court thus acted on the basis of a plea that would have been totally unpersuasive to a non-aboriginal tribunal. But if the outcome was just in that community, one can understand the increasing interest among Canadian aboriginal communities in having their own justice systems reestablished.

Contemporary terms in the Inuit language articulate the full range of legal terms employed in court procedure. *Qanercetaarvik* ("where one is made to tell the truth") and *apiqsuivik* ("where one is questioned") are examples of contemporary *Inuktitut* terms. But one can also find *Inuktitut* terms and expressions extant before the arrival of the *qallunaat*, suggesting a clear understanding of crime and punishment. Some appear in the Eskimo-English Dictionary, which happened to be published in 1925, a year after the decision at Parketuk. These terms appear in Table 22.1.

Table 22.1 / Inuktitut terms which illustrate an understanding of crime and punishment

Inuktitut	English
<i>illissimaneq</i>	wisdom, knowledge, prudence, sense
<i>illuseq</i>	custom, habit, practice, manner
<i>kattimavik</i>	meeting place
<i>nalegiviq</i>	gathering place auditory
<i>okalugvik</i>	a gathering place where one heard speakers
<i>kachitorlek</i>	meeting place of the council
<i>kachimiovik</i>	a place, time where and when men gathered to deliberate
<i>kachitorlek</i>	the meeting place, house, structure where the council met
<i>kachimiuk</i>	the council meeting of the powerful ones
<i>erkartuivik</i>	court, place of execution
<i>passiva</i>	he brings an action against him
<i>passiksernek</i>	a charge, an accusation
<i>passijaksak</i>	the one who is to be accused
<i>passijak</i>	the accused
<i>passijauvok</i>	he is accused of
<i>passijaunek</i>	being accused of
<i>sinnaunganek</i>	the cause of the offense
<i>terliarkattainek</i>	slander
<i>idluitullinek</i>	offense, misdeed, crime
<i>tokkotsinek</i>	killing
<i>pijugakpa</i>	he wishes to kill him
<i>pijugaklerpok</i>	he attempts to kill him
<i>pikkablajoksovlutik innugmik tokkotsivut</i>	they killed a person while they did evil things
<i>tokkopa</i>	he kills him
<i>tokkorartipeit</i>	he permits them (several) to be killed
<i>tokkotita</i>	one who has been killed by permission (the victim's?)
<i>tokkoviksak</i>	where and when one shall die
<i>tokkungajok</i>	a dead person
<i>tokkotak</i>	a murder victim
<i>unalek</i>	murderer
<i>innuartok</i>	murderer
<i>idluitullijok</i>	an evil doer
<i>inuk tamma erkartuivigivara illuserivalauktanganik</i>	I put this man in mind of his former conduct
<i>erkartuijotiksak</i>	an old thing that must be born in mind
<i>erkartortiga</i>	my judge, who put me in mind of old things done
<i>erkartuivigiva</i>	he judges him
<i>erkartortak</i>	one who is judged, sentenced
<i>erkartuivik</i>	court, place of judgment, execution
<i>pitlartaujut</i>	the reason, grounds, cause of the punishment
<i>pitlaraksak</i>	one who deserves punishment
<i>pitlaraksauvok</i>	he is punishable
<i>pitlartauniga</i>	his punishment
<i>pitlarak</i>	a punished one
<i>pitlarniktok</i>	the one who inflicts the punishment
<i>pitlarninga</i>	the punishment that he inflicts
<i>pittarniut</i>	the instrument used to punish
<i>sugiartak</i>	one who is punished
<i>pittaraksauungilak</i>	he is not punished
<i>saimarvigijauvok</i>	he will be or is to be pardoned, or one is satisfied again with him
<i>saimarvikaunek</i>	the act of pardoning
<i>saimarvigijaurik</i>	the place or time where and in which grace is exercised
<i>saimarvigijauninga</i>	the pardon, grace, which one receives
<i>saimarviovok</i>	he is pardoned
<i>takkopsoarneq</i>	mercy, charity
<i>nalektauvok</i>	the one who is obeyed
<i>nalekte</i>	the one who obeys
Other Inuktitut terms relevant to this account are:	
<i>Qallunaat</i>	white people
<i>Qallunaq</i>	white person
<i>naalaktuq</i>	relationship based on seniority and obedience
<i>ungajuq</i>	relationship based on affection and cooperation
<i>angajugqaaq</i>	camp boss, decision maker, respect gained through experience
<i>issumataq</i>	camp boss, decision maker, respect gained through intellect, judgment
<i>issumaliuqtiit</i>	the wise men
<i>pimariit</i>	those who were powerful speakers, who chose words well and could arrange them in amazing ways.
<i>tapananitiit</i>	the powerful ones, deserving of the highest respect

Note: Many of the terms above are to be found in Rev. Edmund J. Peck's *Eskimo-English Dictionary* (1925).

Postscript

The sequence of events did not end with documenting images and words illustrating the last known Inuit trial on southwest Baffin Island. On January 14, 1992, I was informed that The Honorable Chief Justice Antonio Lamer, Supreme Court of Canada, would circulate a copy of *The Last Traditional Inuit Trial on Southwest Baffin Island* to his colleagues and deposit it in the Supreme Court Library for future reference.

More recently, in a letter dated February 16, 1994, The Honorable Judge Jean-L. Dutil wrote to me in response to reading this paper: "It is of great interest to me, as it is the basis for the circle I hold in the north. This reference is to a growing practice of involving native communities in sentencing and the justice system."

At a time when aboriginal peoples in Canada are advocating a return to their own systems of justice, it is particularly appropriate that the disclosure of the site of a court-like structure at which decisions were made on life and death and other important community issues on Baffin Island be revealed. Though the actual location is not revealed, the type of case that would have been heard there is disclosed in this deeply touching account of death, deliberation, and decision in the Canadian Arctic.

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I am deeply indebted to Osuitok Ipeelee, my mentor, who continues to share his experiences of days past. Pauta and Pitaloosie Saila took me into their confidence and showed me things of extraordinary power and beauty. Annie Manning opened doors that would have otherwise been closed. Her sensitive and penetrating way of conveying the meaning of words is truly outstanding. I thank Alan Grant, Professor of Law at Osgoode Hall, whose insights from investigative and prosecutorial/defense perspectives add much to this paper. Thanks to my old friend Tuniksiuti, commonly known as Dr. William Taylor, Jr., for his valuable guidance. The help of Darrell Eagles has been invaluable in

preparing this and other papers. My old friend Terry Ryan, who has spent the greater part of his life in the Arctic, continues to be as interested and hospitable as he was when we first met thirty-five years ago. My sincere thanks to Robert Jarvis Q.C. for his interest and support over the years.

Notes

1. Pauta and Pitaloosie Saila, internationally renowned artists, reside in Cape Dorset. Pauta is a highly respected elder who retains much knowledge of traditional Inuit life.

2. Osuitok Ipeelee is an internationally renowned artist, member of the Royal Canadian Academy of Art, and widely traveled. He possesses an extraordinary amount of information about traditional Inuit life.

3. Cape Dorset (64°14' north latitude, 76°32' west longitude) is a community of approximately 1,200 people situated on Dorset Island off the Foxe Peninsula in southwest Baffin Island. Known as *Kingait*, meaning the high hill, it is famous for the number of internationally acclaimed Inuit artists who live there. It was from Cape Dorset that artifacts of an ancient people, who flourished between about 800 B.C. and A.D. 1400, were sent south. They were identified by the late Diamond Jenness, the famous anthropologist, as belonging to a distinct way of life, which he named the Dorset Culture and which was spread widely across Arctic Canada.

4. Men rose to the position of camp boss by excelling in a merit system in which knowledge, skill, and judgment exercised with great effectiveness were recognized with leadership. They had total authority over the camp and were replaced only when they lost their faculties.

5. This is a touchy subject. They were quite often an orphaned child, a person with a disability, or a young person adopted from another camp. Performing the most menial chores and on occasion subjected to harsh treatment, their status and treatment were similar to those of the serfs of Central Europe. Ironically, some of the best and toughest camp bosses were once camp slaves.

6. *Umiaq* (pl. *umian*) is a large, seagoing boat made of wood and hide, and equipped with a small square sail. Because it was rowed by women, it was sometimes referred to as the women's

boat. It was capable of carrying several families and their belongings, and was sometimes used for whale hunting. Believed to have been developed by the Thule culture Inuit, it was once used throughout the Arctic from the Siberian coast to Greenland. The term *umiaq* was also used to denote other large boats, such as Peterheads, trap boats, whale boats, etc.

The *umiaq* referred to in this account is the Hudson's Bay Company's supply ship, S.S. *Nascopie* that serviced Lake Harbor and *Amadjuak*, among other places, on its way to Cape Dorset. The *Nascopie* sank just off Cape Dorset in 1947 after hitting an uncharted shoal.

7. It is preferable that I apologize to the reader for using only the initials of people who are deceased, rather than having to apologize to their surviving kin for revealing their name to strangers without permission.

In traditional times, the conferring of names was often an act of great importance. In this connection, the significance of the following has eluded me; perhaps someone may understand its meaning. At the trial of *L*, one of the council members was *Q*, a prominent and highly respected shaman. Years later when *Q* had a son,

he bestowed upon that child the accused person's name—*L*.

8. Commenting on this paragraph, Professor Alan Grant noted that, "It shows the council as triers of fact familiarizing themselves with the scene, rather like a modern jury taking a view, as it is called, when they go to the scene of some very important event in a case. This is very seldom done in criminal cases now, but is still possible. In fact, in early English legal history, the jury was not picked from those with no knowledge of the case, but from those who had express knowledge of the case. Witnesses were then called to support the reputation for veracity of different members of the jury. It was only much later that a jury was chosen from those with no connection with the events to be tried."

9. The person *P* was the last of the great camp bosses in southwest Baffin Island and one of the *tapananitiit* who was known throughout the island. He was a person of legendary qualities. The Hudson's Bay Company was so impressed with his abilities that they sent him to communities all over Baffin Island to improve trapping and the preparation of skins. Later, with the coming of the missionaries, he became a renowned catechist.

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